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Note: "The College President's Wife" by Walter Crosby Eells, announced for publication in this issue of *Liberal Education*, will appear in the issue of October 1961.

Foreword

This issue of *Liberal Education* consists of five papers that were given orally, in shorter form, at the 47th Annual Meeting of the Association of American Colleges, held at Denver, Colorado, on 11 and 12 January 1961.

The theme of that meeting was "The President and the Academic Disciplines." It was planned by the Board of Directors as a means of coming to grips with one of the most serious problems of higher education in the United States—the progressive divorce of academic administrators from the actual business of scholarship and teaching. One aspect of this situation is the common lack among college presidents of a broad, up-to-date knowledge of the state of learning in the several disciplines. The president needs such knowledge in order to provide academic leadership, to take intelligent decisions on curriculum and staffing and to plan wisely for the future of his institution. Yet the growing number and complexity of his administrative duties make it very difficult for him to keep himself so informed.

On the initiative of President Courtney Smith of Swarthmore, the Board decided to try the experiment of choosing six fields of study—two each in the humanities, the social sciences and the natural sciences—and inviting an outstanding scholar in each field to review for our members the recent development and current state of his particular discipline. The cost of the experiment, including publication, was covered by a grant generously made to the Association by the Carnegie Corporation of New York.

The disciplines chosen for review were English and philosophy, economics and political science, mathematics and physics. With the help of a number of learned societies, professional associations, philanthropic foundations and knowledgeable individuals, to whom we acknowledge a deep debt of gratitude, the Board chose the following scholars to report on their respective disciplines:

Douglas Bush, Gurney Professor of English Literature, Harvard University
Brand Blanshard, Sterling Professor of Philosophy, Yale University

Ben W. Lewis, Avery Professor of Economics, Oberlin College

David B. Truman, Professor of Government, Columbia University

Marshall H. Stone, Andrew MacLeish Distinguished Service Professor of Mathematics, University of Chicago

John A. Wheeler, Professor of Physics, Princeton University

The five papers that follow need no commendation beyond their authorship and their intrinsic merit. (The sixth was unfortunately not available for inclusion in this issue). The shorter versions were so well received at the Denver meeting that the Board of Directors was left in no doubt about the value of our experiment. We take pride and pleasure in presenting these papers to a wider audience, in the confident belief that they constitute a significant contribution to the Association's constant aim of improving the quality of higher education.

THEODORE M. HESBURGH, C.S.C.
President, Association of American Colleges

Literary Scholarship and Criticism

DOUGLAS BUSH

I am happily conscious of the honor of being invited to discuss the purposes and problems of scholarship, criticism and teaching in the field of English literature.

Before we get into our large subject I should like to raise a still larger question which may be in our minds as we go on to look at particulars. A special difficulty confronts anyone who speaks about literature in what is *par excellence* the age of science, an age of achievements both dazzling and menacing. Apart from threats of entire extinction, some scientists, including psychologists, and many laymen seem to assume that science holds the answers to all or almost all problems, individual and social; one even meets the idea that in the not very distant future surgery or pills or mere brain-washing will make people good and eliminate evil. And we hear on all sides silly slogans like "Education for the space age," as if exploration of space had anything to do with the quality of life.

I hasten to say that I am not setting up as an enemy of science, which would be idiotic. Everyone is aware, daily and hourly, of the infinite benefits of applied science and of the less tangible benefits of pure science; and the attaining of knowledge and control of the material world is a noble end. All that is self-evident. I am speaking only of still more central constituents of human nature and life which must not be pushed or allowed to sink into a secondary place. We may agree that there should be no antagonism, but the historical fact is that there has been and is. In the seventeenth century the beginnings of the scientific conquest of nature made this question a reality, and in that age many men, from Donne to Milton, opposed such enquiries if they obscured or perverted man's sense of the true ends of life. What Christians called pride the Greeks called *hubris*. The stories of Prometheus and Faust remind us that the quest of knowledge and power has its sinister as well as its noble side. There have always been voices raised against excessive or exclusive claims made for science, voices that affirmed other views of the possibilities and the limitations of man. And with them have been joined the voices of those scien-

tists who have not been deceived by illusions of total progress, and I might quote some representative words from one of them, Erwin Schrödinger. After noting that the scientist achieves a simplified understanding of nature by cutting himself, the human personality, out of the picture, he goes on to say, in terms that are partly close to Donne's, that that picture of the real world is very deficient:

> It gives a lot of factual information, puts all our experience in a magnificently consistent order, but it is ghastly silent about all and sundry that is really near to our heart, that really matters to us. It cannot tell us a word about red and blue, bitter and sweet, physical pain and physical delight; it knows nothing of beautiful and ugly, good or bad, God and eternity. Science sometimes pretends to answer questions in these domains, but the answers are very often so silly that we are not inclined to take them seriously.[2]

As I said, I am not foolishly opposing science. I am opposing the not uncommon notion that all truth, all reality, is contained in science, and the concomitant notion that literature and the arts are only a cultural shampoo, a cocktail for an hour of relaxation. From the beginning to the present, it has been chiefly literature and the arts that have embodied the ethical imagination, the conscience, of mankind.

One word more by way of preface follows naturally here. As the first half of this paper will, I hope, suggest, discovery—discovery in the factual but much more in the ideological sphere—is a part of humanistic study; if it were not there would be stagnation. Yet discovery, even in the world of ideas, is not, as it is in science, the whole or even the main objective. Great authors produce great works of art because, given their genius, they have imaginative, emotional, moral, religious experience which they must express and communicate. The richer and more complex such works are the more they are in need of interpretation; and one major difficulty is the fact of the pastness of most of them, of their being scattered over three thousand years. The *sine qua non* of scholarship, criticism and teaching is the effort to understand, and to help others understand, these works, to make them available to successive generations, to make the authors' re-creation of their experience an enrichment of our own. The age of a work of art has of course nothing to do with its present and universal value. A great artist's vision of life is never out of date, however much interpreters may differ or change from period to period.

The initial and the final fact about our topic is that English literature

(including American) comprises a very large body of great writing which is our chief cultural heritage. All people who consider themselves educated need to possess as much of it as they can; they are mentally crippled if they do not. But it is a fair guess that ninety per cent of the educated public has no interest in great literature, and this almost complete indifference is reflected in the popular press; literature is only a part of the college curriculum, not a part of life. What is or can be assimilated in college is a small fraction of the whole, and it might be wished that college courses were regarded as a beginning rather than an end. It is one of the painful facts of the American way of life that we are not a nation of readers; we lag behind a number of other and smaller nations in our appetite for books, and, although we have a far higher proportion of young people in college than any other, for a high proportion of these a liberal education seems to end on Commencement Day. Even starry-eyed undergraduates can turn into glassy-eyed alumni and alumnae who, if they read anything, read best-sellers and magazines. But we must plunge into our subject, which has many ramifications. The word "English" in a college or university syllabus covers wider and more diversified functions than any other departmental heading; departments of English are supposed to do everything from spiritualizing the community to teaching all freshmen the rudiments of their native language.

In the first part of this paper I shall try to describe the various approaches to English literature as these have developed in the scholarly and critical writing of the last fifty or sixty years. These approaches have been extensions of or, more often, reactions against those that prevailed in the late nineteenth and the early twentieth century. Though I may use the past tense, because the sequence is partly chronological, it is to be understood that all these modes of study continue in active operation. Since the paper is not a history but only a rough outline, I shall stick pretty much to the American scene.

The early twentieth century produced a number of critical writings that retain high value, from Henry James's prefaces to A. C. Bradley's *Shakespearean Tragedy*, but the mass of criticism of that period may be given the label of impressionism. It had the virtues and defects the label implies. A person of some knowledge and of cultivated taste, but with no special learning and no special doctrine, recorded his response to a given work or author. The intuitions of such a critic might or might not be suggestive and valuable. Quite often, as in the writings of the omniscient or at least omnivorous George Saintsbury, the reader felt the critic's in-

fectious enjoyment and gusto, which was a good thing; but he was not likely to acquire many ideas. Allowing for distinguished exceptions, we might say that this criticism in general was a kind of high-class literary journalism, akin to reviewing. Recalling Saintsbury's cellar-book, we might think of the impressionist critic as a wine-taster with an unusually sensitive palate.

At the opposite pole from such criticism was the kind of literary scholarship that was not only developing but dominant in the United States. It is sometimes called German, because German scholars had led the way, and sometimes scientific, because it applied to the study of literature something of the method that had been so fruitful in nineteenth-century biology. Its objects and methods were historical, but that word is too broad to be useful. For our purpose here we might use the term "genetic." In brief, genetic scholarship saw advanced literary study as the discovery of sources and the tracing of relations, purely literary relations, among authors and works. The favorite area of investigation was medieval literature, which had been relatively neglected. As Stuart Sherman said, in his once-notorious portrait of G. L. Kittredge (in 1913), "from the James River to the Golden Gate, all America mediævalizes." I remember that at my own oral, in 1923, Mr. Kittredge, in a facetious moment, asked me to name the wildest Chaucerian in America outside the room we were in—there being five or six eminent Chaucerians in it. (I must add that these reminiscences carry no reflection upon the unique and incomparable Chaucer, who dwarfs most of our contemporary demigods.) The literature of all periods was studied in much the same way, through the tracing of sources and influences. To disregard theses, in which such research was likely to be external and mechanical, and rise to the work of a master, there is such a monument as *The Road to Xanadu* by my own father-confessor, John Livingston Lowes, whose passion for literature fired and sustained many young minds.

This kind of scholarship has often been disparaged, but it must be said that it did rewrite the history of literature on a new level of broad, minute and solid learning; and that was a great achievement, to which all later scholars and critics remain indebted. Also, even the humblest practitioners had to possess exact knowledge and a faculty for weighing evidence. On the other hand, this kind of scholarship commonly fought shy of aesthetic or ethical criticism (that was the ultimate purpose, but for someone else to carry out), and it gave what was pretty often a dusty answer to those students who had supposed that literature would mean

something more to them as human beings. Intellectual and—if the word is permitted—spiritual ripening did not seem to be fostered by external studies of influence or of authors too small to have been studied before, or by the tracing of a folklore motif from Pennsylvania to Polynesia. I myself, during my two years of graduate work at Harvard, was kept so busy with courses in linguistics, Gothic and so on, that I had few glimpses of literature at all.

Although many students in graduate schools murmured against this kind of study and research, perhaps the first conspicuous rebel was Irving Babbitt, professor of French at Harvard. Babbitt was of course a moralist with a message. Combating the expansive romantic emotionalism which was to him the root of all modern evils, he set up a rigorous ethical rationalism based on his reading of classical and oriental thinkers (along with a perhaps unconscious layer of New England Puritanism). Through his books and his strong influence on prospective college teachers, and through the influence of such allies as Paul Elmer More, the so-called "New Humanism" became news in the 1930's. Whatever the aesthetic deficiencies or the moralistic excesses of this movement, it was deeply concerned with literature as something that matters and is not merely material for scholarly investigation. Thus, in addition to his ardent disciples, Babbitt attracted and stimulated minds more or less opposed to his ethical gospel, such as the late F. O. Matthiessen or T. S. Eliot; the latter, though respectful and grateful, found Babbitt's non-religious doctrine quite inadequate.

Another rebellion against orthodox literary scholarship likewise began before 1920 and has itself become an increasingly fruitful orthodoxy. I refer to the history of ideas. Whereas what I have called genetic scholarship was wholly or mainly concerned with literary facts and relations, the newer kind of history was concerned with ideas as a means of interpreting literature. Babbitt might be called one of the pioneers, but he always had an ethical axe to grind. The disinterested study of ideas began especially with the philosopher Arthur O. Lovejoy and his disciples, such as Marjorie Nicolson, and with such literary students of the Renaissance as Edwin Greenlaw, James Holly Hanford and Allan H. Gilbert. Professor Lovejoy brought a philosopher's equipment to the analysis of basic currents of thought and feeling, primitivism and romanticism for example, and he stimulated many other inquirers. Miss Nicolson has given much of a full career to studying the impact of early modern science upon literature of the seventeenth century; and during this last genera-

tion a crowd of others have explored the relations of literature and science, so that there is now a whole library in this area. Since scientists are so much given to complaining of humanists' ignorance and neglect of science, it might be remarked that nearly all these books and articles have been written by literary scholars (scientists have of course done much on the history of science itself). We have learned a great deal about the effects of science upon thought and literature over many ages, and have long been able to take such knowledge for granted, so that it is hard to realize that fifty years ago the subject was almost unknown and laymen's understanding was commonly limited to a few half-fallacious and inert generalities.

Another and still more complex field for literary students of ideas has been philosophy and religion. Lovejoy's *The Great Chain of Being* (1936) was a seminal work that illuminated whole bodies of literature and inspired further illuminations. The other pioneers I mentioned, Greenlaw, Hanford and Gilbert—the latter two, with Hardin Craig, are happily alive and active—were students of Renaissance humanism as this was expressed by Spenser, Milton and other writers of poetry and prose. In this field laborers and gains have also multiplied. Both general movements and individual writers have been studied in relation to ancient, medieval and modern philosophies and to the various branches of Christian belief and thought. For instance, Puritanism in England and New England has been really understood and explained by such interpreters as M. M. Knappen, William Haller, A. S. P. Woodhouse and Perry Miller.

In short, the modern teacher and student of English literature of the sixteenth and seventeenth centuries must know as much as he can of classical, medieval and Renaissance literature and thought, of the Bible and its commentators, of St. Augustine and other church fathers, of Aquinas and Ockham and Calvin, of Copernicus, Kepler and Galileo, of Bacon, Descartes and Hobbes, and many other thinkers and writers outside as well as inside literature, and of course political, social and economic history and the fine arts. Later eras bring corresponding needs. No literary scholar or critic can fulfill such requirements, but one has to keep on trying; the necessity is visible, as it was not fifty years ago, when literature and some general history were usually enough. And intellectual and cultural history of this kind is itself a discipline of major importance.

I said that genetic scholarship had rewritten the history of literature on a new level of literary knowledge; and students of ideas have in their turn

rewritten the history of literature in other and more philosophic terms. The value of the history of ideas is sufficiently demonstrated by even a brief description. But no one approach to literature is adequate by itself, and this approach, like the study of literary sources, has one evident shortcoming: it contains no criteria of value. A work of art, however philosophic or religious or scientific it may be, is still a work of art and not a philosophic treatise, and, so far as this method itself goes, a poor poem or play or novel may, as a mirror of ideas, be no less interesting than a great one. The corrective is of course in the fact that the individual student does have and can use criteria of value. Granted that obvious fact, however, the study of ideas could, like literary history, build up backgrounds and neglect the particular work of art. And that is where the so-called "new criticism" came in.

This movement was well under way before the label was established by the title of John Crowe Ransom's book of 1941, a group of essays on the critical writings of I. A. Richards, Richards' pupil William Empson, T. S. Eliot and Yvor Winters. These men and such others as Mr. Ransom himself, R. P. Blackmur, Allen Tate, Kenneth Burke and Cleanth Brooks, were united chiefly in their effort to see the individual work of art itself, to analyze its texture, diction, images, structure and rhythm, to show how and why it worked on the reader. In essence, this "new" method of rhetorical analysis was some two thousand years old, but the habit of close reading had declined with the decline of classical education, and modern readers, who had grown up on impressionism, had lost a good deal of the capacity to read English with the active sensitivity great art demands. One general service the new criticism performed was to teach a generation of slack readers that loose impressions will not do, that for adequate comprehension of works of art all one's faculties must operate at concert pitch.

The diversity of background and outlook among the new critics may be illustrated by a glance at the two men commonly regarded as the pioneers, the Anglo-American T. S. Eliot and the English I. A. Richards. Both men, like the American Ezra Pound and the English T. E. Hulme, were rebelling against a now outworn romantic tradition in poetry and against thin, vague, unprincipled impressionist criticism. Mr. Eliot's critical essays, in their gentlemanly amateur manner, might be said to have risen from, and above, conventional impressionism by virtue of the fresh, acute and suggestive insights of a craftsman of signal originality and subtlety. He gained stature simultaneously as poet and as critic, and at a

very early age exercised something like papal authority on both sides of the Atlantic. More than any other individual, he turned sophisticated minds away from the romantic tradition to the poetry and drama of the earlier seventeenth century (plus Dryden) as the standard of good writing; and the effect, on modern poetry as well as criticism, was immense. However, from our present standpoint, it may be said that, while Mr. Eliot's criticism was a radical and mainly beneficial stimulus, a good deal of it, like that of many poets of strong individuality, was an oblique defense of the way of writing that was right for him. Thus, as criticism, it often lacked objective validity, and it is only in recent years that Mr. Eliot's interpretations of seventeenth century poetry have been revised by more historical scholarship and criticism.

Mr. Richards, though he disclaims the role of co-founder of the new criticism, had a very large share in it. (Incidentally, in his later years he has turned poet himself.) A psychologist by training, he was scientific, systematic and rigorous in the books he wrote while still teaching at Cambridge, *Principles of Literary Criticism* (1924) and *Practical Criticism* (1929). These and other writings supplied both a philosophized creed and a method of close critical analysis, and their great influence went along with Eliot's in changing the critical climate and the aims and procedures of criticism. The new doctrine developed more rapidly and widely in this country than in the country of its origin, so that before long no department of English could call itself respectable unless it had at least one card-carrying member of the new school; and very many of the younger generation turned away from conventional scholarship to critical analysis, with mixed results.

Before we leave the pioneers, a word may be said on one large question: that is, why do we read and study literature at all, what is the ultimate aesthetic, moral and spiritual benefit? For practitioners of the older literary or genetic scholarship these benefits, if recognized, were taken for granted, at any rate not defined; and, as I said before, many eager novices felt that they were being given stones in place of bread. The history of ideas was much less unsatisfying, yet even that did not make great works of literature a personal possession. The new criticism, if at times it seemed to be content with the cool intellectual detachment of a mathematician, inevitably came closer to the root of the matter. In 1880 Matthew Arnold, seeing no future for the supernaturalism of the Christian faith, had affirmed that poetry must now take over the responsibilities traditionally fulfilled by religion; and a good many students of

literature more or less shared that view. Mr. Richards, a sophisticated and sceptical psychologist who yet desired a firm basis for imaginative art, may be said to have restated Arnold's position; distinguishing between the verifiable statements of science and the affective pseudo-statements of poetry, he found the value of the latter in their power to organize our attitudes. Mr. Eliot, on the other hand, as a devout Anglo-Catholic, rejected the Arnoldian view and saw Christian truth as the absolute; poetry —presumably even religious poetry as great as his own *Four Quartets*— has a much lower place in the hierarchy of values. The major American exponents of the new criticism have all been concerned about the problem of value; speaking with diffidence on this point, one might say that some appear to be on Eliot's side and more on that of Arnold and Richards. At any rate they have been hostile to excessive claims made for science and have insisted on the "cognitive" validity of poetic insight and art.

Because of the diversity of interests, outlook, aims and methods among the new critics, it is hard to generalize and to draw up a brief balance sheet of virtues and defects. Though they all hold or have held academic positions, they are men of letters, some of them poets, who leaven the scholarly dough the rest of us compose. They have been more openly conscious of the state of the world than scholars generally allow themselves to be, and they have done much to bring the study of literature closer to men's business and bosoms, to make it recognized as a central need and resource in a confused and troubled age. Further, as we have also seen, they have taught us to read closely, to become sensitive to all the interacting components of a piece of writing, and to replace loose impressions with precise analysis. These are large virtues, which outweigh the deficiencies that take a bit longer to rehearse. One deficiency is that, in reacting against scholarly concern with backgrounds, the new criticism has gone too far in the opposite direction. It has in theory, if often not in practice, insisted that a work of art is an isolated, self-contained entity which must itself supply the answers to the questions it raises, that the critic must not go outside it to import evidence. This is a very arbitrary fallacy, since a critic or reader obviously needs to use any and all kinds of evidence, historical, biographical, ideological, linguistic, that contribute to understanding. Although many particular explications of individual works have been of high value, a good many are more or less damaged by this unhistorical or anti-historical attitude. For it is surely a fundamental axiom that every work of the past must be read with

bifocal lenses; that it must be understood first on its own terms, in relation to its creator's historical milieu, language and intention; and that, while every great work is great because it transcends the local and temporal, it cannot be understood if the reader brings to it the very different preconceptions of his own time. Interpretation without adequate knowledge is bound to be misleading, though of course the degrees of necessary knowledge vary widely; clearly we need much more for Shakespeare and Milton than for Jane Austen and Trollope (though even the latter can suffer from misapprehension).

Some other charges have been lodged against the new critics. For several decades the fever of critical explication has been so contagious, and has brought out such a rash of jargon, that some people have been afraid lest highly self-conscious intellectualism extinguish common humanity along with the luminous decencies of language. Whereas the Saintsburys had addressed the general reader, modern criticism is largely a highbrow preserve in which the general reader may feel lost. (On the other hand we might recall what I more or less said at the beginning, that our age seems to have witnessed the virtual disappearance of the general reader of literary interests, so that the critic's serious audience is very small.) Then, too, the techniques of analysis can encourage over-emphasis on particular kinds of poetry or particular elements in poetry, as if these were of prime and universal importance. For example, the new criticism has made a great deal of imagery while often neglecting other elements. It is significant that the new criticism has been at its best in dealing with short poems and has commonly slighted the epic, drama and novel. It may be added that some scholars, trained in the older scholarship, have combined historical learning with the aims and insights of the new critics; one notable exemplar is Rosemond Tuve.

The loose label "social criticism" may serve for several different kinds and grades of writing. A generation ago it meant proletarian or Marxist criticism, which commonly crowned or decapitated writers according to their economic and social views. That stream, which was popular rather than academic, seems to have dried up. Another stream of equal naïveté unfortunately has not dried up, to judge from such representative educationist statements as books of the National Council of Teachers of English, *The English Language Arts* (1952) and *The English Language Arts in the Secondary School* (1956). Their critical standards may be indicated by a single sentence in the later one which groups together, as illustrative of family relationships, *Alice Adams, Of Human Bondage,*

Sorrell and Son, Hamlet and *Antigone*. In these books for teachers literature is seen, in spite of some disclaimers, as an appendage to social studies. One does not know if millions of high-school pupils are brought up on this distorted and uncritical doctrine, but one has horrid fears.

On a very different plane, the word "social" may perhaps be used to distinguish the criticism of Lionel Trilling. Mr. Trilling's first book, his study of Matthew Arnold, foretold his own general direction; he has shown a broad Arnoldian concern with both literature and social and moral culture. I do not know any single word or phrase that would serve for the dean of American critics, Edmund Wilson. He also has a social outlook, along with diverse literary and historical interests, but one can hardly generalize about his critical work except to say that he has been an acute and forceful champion of good writing and an enemy of bad, that he has not been attached to any school, and that, like Mr. Trilling, he addresses that forgotten man, the general reader.

Modern psychology, of course, has more or less conditioned all modern criticism. Freudian ideas were rapidly utilized by imaginative writers, sometimes in clinical fashion, and critics of modern literature necessarily followed suit. But such ideas could be found everywhere in the older literature as well, since Freud was defining permanent elements of human nature. Some ideas, such as frustration, became master-keys for opening all doors. A crowd of authors and characters were seen trudging along the road back to the womb. Along a parallel road stumbled another crowd driven by the death-wish. Since *Moby Dick* has been a special target for critical theories, one might add the suggestion that the white whale represents the Spirit of Literature turning and rending one-legged critics. While some psychological criticism has been notable, most of it seems to me dubious, whether carried on by literary critics or psychologists. One example is the analysis of *Hamlet* by the psychologist Ernest Jones.

One approach to literature which has of late had a wide vogue is the myth-and-symbol doctrine. I spoke of the new criticism as some two thousand years old, and the poetic use and critical recognition of myth and symbol are older than that; the allegorizing of myth began before Plato and has in some form been operating in European poetry and criticism ever since. But we are concerned with the modern movement, which started from anthropology combined with Jung's psychological theory of the collective unconscious or racial memory. The first critic who systematically applied the theory to literature was the English Maud

Bodkin in 1934. In brief, this doctrine rests on the concept of archetypal myths which embody certain fundamental and universal human experiences and beliefs, such as the journey or quest, the passing from youth to maturity, the revival of nature in the spring, the death and rebirth of a god, and so on. Such stories exist in all times and places and are told and retold on various levels of sophistication. For instance, William Faulkner's famous story, "The Bear," deals with the transition from youth to maturity. While the modern concept of myth is much more comprehensive than the word "mythology," we naturally think first of Greek myths and of such archetypal figures as Odysseus and Prometheus; or, in other spheres, of Satan and Faust.

The latest and most elaborate exposition of the whole theory is *Anatomy of Criticism* (1957) by Northrop Frye of Toronto; here all the kinds and dominant themes of literature are defined, classified and illustrated with equal learning and brilliance. One particular application, which shows how valuable the method can be when used by a perceptive and rational intelligence, is Isabel MacCaffrey's *Paradise Lost as "Myth"* (1959), which was originally a Radcliffe thesis. I included the word "rational" because many exponents leave reason behind. (I understand that modern biblical criticism is in a somewhat similar state.) The hunt for symbols has put many undergraduates and graduate students under such a hypnotic spell that it is vain to plead for common sense. It is doubtless in compensation for the lack of religious faith that, for many of the young and not a few elders, any fruit encountered in a poem must be an allusion to the fall of man, any tree an allusion to the Cross. Some years ago I was stirred by this epidemic to write a parody in the form of an analysis of archetypal myths in *Pride and Prejudice*. To return to higher levels, this approach to some kinds of literature is thoroughly valid and richly illuminating, as indeed the antiquity of both the poetic and the critical method might guarantee; and the doctrine cannot be blamed for the fantastic operations of some devotees. But the doctrine does appear to have one large deficiency. While it gives works of literature their place in universal patterns and thus enriches their significance, it, like the history of ideas, has in itself no criteria of value. Of course its exponents, like historians of ideas, may and do have such criteria.

Some other approaches to literature must be noticed more briefly. Formalism, which has been especially identified with scholar-critics at the University of Chicago, may be described as a modern and highly sophisticated and rigorous version of Augustan neoclassicism. It has been

preoccupied with both theory and the application of Neo-Aristotelian principles to the structure and logic of works of art. Works of art are "made objects," "productive of definite effects upon our minds," and they are to be distinguished and analyzed as wholes in terms of the generic and specific causes of those effects. Hence the Chicago critics are severe upon all modes of approach that are predominantly subjective, or social, or psychological, or that start with language or imagery or symbolism.

My lack of both knowledge and time forbids anything more than mention of the new and growing science of structural linguistics. Its primary object is analysis of language by way of units of sound rather than in traditional grammatical terms. Some linguistic scholars appear to be quite impervious to literature, but others have begun to use the new tool in the study of poetry. What little criticism of this kind I have seen did not bring a revelation, but I am told that the method has been and can be fruitful.

Finally, there is the new science of technical bibliography. It has logical priority over all kinds of literary criticism, since its central purpose is the establishment of accurate texts. The new bibliographers have not only dealt with traditional problems, such as those presented by Shakespeare, but have exposed the inaccuracy of many texts that had been taken for granted, including those of some American authors. Thus, while the average Victorian editor was little given to collation, and, if confronted with variant readings, might simply choose the one he liked best, the modern bibliographer seeks to discover what the author actually wrote. This effort requires complex kinds of investigation, which range from infra-red rays to the individual and perhaps alcoholic habits of Elizabethan compositors.

We have looked through the colors of the scholarly and critical spectrum, which, I might add, are all reflected in the changing character of the learned journals. In the mind of a theoretically ideal critic, these colors would all blend in the dazzling white light of truth. But there is of course no such critic, and we must get on with what limited talents we have. Besides, even if there were such an ideal critic he would not be a final one, simply because neither the creation nor the criticism of literature is an exact science. These approaches to literature have perforce been described separately, but individual scholars and critics do not follow one line and exclude all others; they follow as many lines as their subject requires or their talents allow. As we have repeatedly observed, no one approach is adequate. The most acute critical intelligence is at sea

if he does not understand the words and ideas of his author; and the most learned historical scholar is at sea if he does not understand the nature of a work of art. Then if we look at our spectrum from another angle, we see that a single author, Shakespeare for example, brings into play all these scholarly and critical activities, from the genetic to the bibliographical. These various schools and methods have carried modern knowledge and understanding to depths never glimpsed before, or at any rate never set forth in print before, and we must be profoundly grateful for such achievements and illuminations. Now and then, perhaps, an old-fashioned veteran may wonder if, amid all this subtle intellectual activity, the ethical function of literature, which for over two thousand years was regarded as central, is not in some danger of getting lost. And in weak moments one may perhaps heave a nostalgic sigh for the good old times when impressionist critics seemed to enjoy the literature they wrote about.

Questions of method become at once clearer and more complex if we turn from the printed page to the classroom. The academic study of English literature is a relatively recent phenomenon, both in England and here. In earlier times, when the curriculum was mainly classical, it was assumed that students could and would read books in their own language by themselves. Nowadays it is part of our national credo that no one can read anything in any field of knowledge without taking a course in it. In the literary area, the usual modern lack of the classics, even of a usable amount of Latin, has created large gaps of several kinds. Up to, say, a century ago, by far the greater part of English literature was written by and for men of classical education (and that holds for a few later writers). It could be assumed that both writers and readers possessed a unified and unifying body of knowledge, a cultivated and homogeneous taste, a habit of intelligent reading, a familiar grasp of English words and idioms and their shades of meaning and association, and so on. Such assumptions have of course long ceased to exist. The majority of students who have entered college in our time—a vastly greater number than before—have read very little literature in their own language, much less in any other; know next to nothing of the Bible or the classics; cannot write a page of good English; and hence cannot read English with perceptive and discriminating taste. And one may surmise, with good reason, that a large proportion of these are only semi-literate when they receive the degree of B.A.

There is no essential difference between written and spoken criticism except—and it is a big exception—that spoken criticism has to be adapted

in various ways to college and university courses. There must be courses on different levels, for freshmen and for graduate students and for those between. In view of the different approaches to literature, there must be courses of different kinds: broad historical surveys of whole periods and of genres within periods, such as the drama and the novel; courses for intensive reading, especially in poetry, since many students have to learn how to read and since poetry is condensed and inexhaustible. In all kinds of courses the instructor will be looking at authors and texts in a number of the ways already described, genetic, ideological, analytical and so forth; the emphasis naturally varies with the subject matter and the level on which the course is operating.

There are people who assume that the number of students in a course need be limited only by the size of classrooms or the efficiency of microphones; and for advocates of television there is no limit. But, as every teacher knows, the larger the audience the more difficult it is to deal with anything complex or subtle, or to make a close analysis of a text; and of course class discussion is ruled out. The impossible ideal is that almost all literary courses should be small, so that members are all attentively involved and can participate, instead of sitting as rows of buckets to be filled. The large lecture course has its ardent defenders, and there are highly successful lecturers; but I cannot think that I am the only person in the world who never remembers anything he hears in a lecture. And it is hard to imagine the state of mind of students who spend their mornings at a succession of lectures on a variety of subjects.

Something may be said at this point about four topics: courses in the humanities, in foreign languages, American literature, and composition. The setting up in recent decades of broad courses in the humanities, the sciences and the social sciences seems to have been inspired by two considerations: that the fragmentary, incoherent results of the elective system, along with the sheer mass of specialized knowledge, made philosophic syntheses desirable; and, secondly, that regular courses for concentrators were not suitable for non-concentrators, who were thereby cut off from essential parts of a liberal education. Such courses in the humanities have sought to give young students a broad and suggestive view of large segments of western literature and thought, sometimes consecutive and classic answers to an enduring question. These courses have, I believe, been mainly successful. Now and then one may be uneasy about the number and the bulk of masterpieces that young people are expected to digest in a short time; so that, to change the metaphor, one

has a vision of a somewhat breathless and bewildered crowd of passengers in the Occidental Express. By the same token one may wonder if intelligent knowledge sometimes gives place to journalistic impressions and opinions. However, at the very least the young are made conscious that significant works and ideas are there and, however old, are greatly alive.

One especially valuable element in such courses is the inclusion of great works from foreign literature, ancient and modern. These are of course read in translation. While there are great translations, from the King James Bible down, it is a plain fact that much foreign literature, poetry above all, has not been and cannot be adequately translated. It is idle to wish for a linguistic Utopia, but things do not need to be as bad as they have been. Greek vanished some time ago from almost all public high schools, and the two years of Latin commonly offered do not provide usable knowledge. In some colleges Latin and Greek can be said to flourish, in that special sense of the word understood by classicists, and it is of great importance that they should be available for the few, as a rule an intellectually distinctive few, who want them. The late Clyde Kluckhohn, the anthropologist, told me, a few weeks before he died, that he and a colleague made Greek a prerequisite for a course in anthropology they were giving and had 69 applicants. We may remember that most great men of the past, including Americans, had a classical education, and, whatever its often faulty aims and methods, I would maintain that in some essential ways it had and has a value which the wealth of literature in English cannot replace.

As for modern languages, during the years when the United States was taking part in two world wars and becoming the leader of the free world, pedagogical authority was busily engaged in eliminating foreign languages from American schools. Of late the press has reflected a growing consciousness of our national shortcomings: the frequent inability of our diplomats to speak the language of the country where they are posted, our failure to understand how other peoples think and feel. For some years the Modern Language Association has been carrying on a campaign; and in some cities languages have been introduced into the elementary schools. All this activity has been to the good. But, of course, the object of foreign-language teaching is not the ability to order a meal in a foreign restaurant. Americans, for all their traveling, have not been distinguished for sympathetic appreciation of foreign cultures; our attitude has often been that of Dr. Johnson toward Roman Catholics: "In everything in which they differ from us they are wrong." To mention

one practical academic problem, year after year able applicants for graduate work and fellowships in English are quite unable to meet requirements in foreign languages; one does not know what they and their college advisers were thinking about.

American literature was for many years kept out of its rightful place in the curriculum of school and college, but in the last generation or two it has made up for lost time. The rapid advance in American studies was encouraged by the second and distinctively American renaissance that was going on during the same period. It no longer had to be admitted that the major writers all belonged to the nineteenth century; some were living among us and were exerting notable influence in foreign countries. American literature of the past and present has of course been studied in all the ways we considered earlier, and there is no time or need to particularize about the new wealth and depth of knowledge and interpretation that recent decades have achieved. But perhaps one or two questions may be asked, not without anticipation of resentment. One has to do with the academic movement away from a prime concern with British literature to what may be thought over-emphasis on American. This movement seems to be endorsed by the National Council of Teachers of English, which represents faculties of education and school systems, and it is doubtless endorsed also by many individual scholars and teachers. While the reasons are obvious, many persons may share the disapproval registered several years ago by Lionel Trilling. Such a movement is simply a turning away from the largest and most central part of our own heritage. Then, proper recognition of the greatness of a number of American authors, old and new, should not bring a loss of perspective; but it may be thought that judicious reassessment has been attended with a degree of inflationary provincialism. Moreover, a good deal of the reinterpretation of American literature has been in the way of cultural history, in which even the smallest writers may have a legitimate place. Such cultural history is highly important in itself, but, for one thing, it cannot be written or understood apart from its European background, and, secondly, the study of cultural history is not at all the same thing as study of the world's greatest writings, from Homer and the Bible onward, and no American writings are in that category. For such reasons it seems to me undesirable to allow concentration in American literature by undergraduates who read or have read little else. In case this remark raises anyone's hackles, I may add that I regret the fact that nine out of ten applicants for graduate work in English have majored in English as undergraduates; it would

often be much better if they had majored in classical or modern languages or history or philosophy or the fine arts.

We come to the troublesome matter of writing. I shall not go into the many obvious and non-scholastic reasons for common illiteracy, but I will mention one. Many school teachers seem to have been nourished on the doctrine that good English is an artificial tyranny imposed by old-fashioned pedants; there is no such thing as a standard of correctness because whatever is used is right. Thus, if two truck-drivers collide, the ensuing verbal exchanges are good English because they are appropriate to the social situation. In general, students in education are at or near the foot of the scale of literacy, so that they can hardly be expected to make their own pupils literate. And nowadays a degree of illiteracy is not uncommon among graduate students of English.

Our concern here is with college teaching. On the lowest level, the English department has the job of serving the whole college and teaching elementary composition to all freshmen, a job which requires a squad or an army of instructors and which cannot be properly accomplished anyhow. We are so used to freshman English that we have apparently lost the capacity for being shocked by it, though there have been signs of revolt in some institutions. We might try to imagine a European university teaching its incoming students the rudiments of their own language—or do we, with some reason, count English as a foreign language? I can see no grounds whatever for admitting illiterate students to college. (We have for some time bypassed that question by not having any test, or any adequate test, of literacy for admission.) We have been hearing and talking about the growing bulge in the college population, but there would be no urgent problem if colleges accepted only those applicants who are qualified for higher education. Appeals to democracy will not alter the facts of nature, and one fact is that intellectual endowments are not distributed equally. Nor is it one of the principles of democracy that the natural rights of every citizen include a B.A. What has been going on during the past generation tells us clearly what the end of the process will be. We have taken it as a matter of course that the first year or two years of college should be given to high-school work; the next step will be that colleges will become glorified high schools, as indeed some are now.

There should of course be ample instruction in writing, since the attainment of real competence is an arduous and never-ending task, but it should be on higher levels. For the mass of students, both in and outside English, the chief need is the mastery of clear exposition, which means of

course clear and informed thinking as well as clear and effective English. This ought to be the conscious responsibility of all college teachers in all areas, but a great many of them seem to shrug it off as the concern of the English department. This is equivalent to assuming that people should be good on Sundays but may do as they please the rest of the week.

The numerous students who want to do imaginative writing, in fiction, poetry and drama, should of course be able to pursue this important interest, in courses on different levels. Even if most of these will not be writers, their desires should have scope, and they will have a better understanding of the literature they read because they can in some degree look at it from the inside. As for the few who will become writers, they always have to teach themselves; but even these can profit from sympathetic encouragement and criticism. I do not think there is much to be said here on this topic, but I will say two things. One is that imaginative writing cannot be taught by anyone who happens to be handy or needs to have his schedule filled out; such teaching requires special talents. Secondly, I do not think students should be allowed to major in "creative writing." Critics of modern American literature have sometimes observed that a number of more or less good writers simply did not know enough, were not well enough educated, so that, after they had exhausted their personal experience, they were empty. Colleges cannot go very far in training real writers, but they can see to it that such persons get some solid education.

I should like to speak of one point which was not among the headings suggested for this survey. That is the principle commonly known as "Publish or perish." I think it is wicked for administrators or departments, which are probably the chief sinners, to crack this whip over young men and women whose first professional years need to be fully devoted to teaching and to reading and thinking of a kind they were not able to do as graduate students. Of course, many people deny that publishing is the main criterion for promotion, but the facts often look otherwise; and in their recent book, *The Academic Marketplace* (1958), Theodore Caplow and R. J. McGee affirm very positively that in our major universities performance is rated "almost exclusively" in terms of publication. This is the academic version of "status-seeking." The results of this widespread policy, or the widespread belief that this is academic policy, are common knowledge: young people feel harassed and driven when they ought to be ripening; many articles and books are produced, under forced draught, which may be more of a burden than a benefit to readers. Problems of appointment and promotion are assuredly difficult, but they cannot be

solved by the counting of the number of pages produced annually. On the other hand it must be granted that a person, young or old, who does not publish is not *ipso facto* a Socrates; he may be an unthinking vegetable. But a teacher's daily associates know if he has or has not an active and growing mind, whether or not he is busily writing. Young people in general are being criticized nowadays for wanting guarantees of security and a pension when they seek their first job. I do not think that young college teachers, who are generally mature and intelligent, take that line; they know they have to prove themselves, and they only want to be judged by well-balanced criteria. This problem of course exists in all departments.

It might be added—if a sinner may venture to rebuke sin—that the pressure or the itch to publish extends through all ages and all ranks, and this is both a cause and a result of what has come to be excessive specialization. The day is long past in which a literary scholar or critic had a real command of the whole body of great literature in English, much less of foreign literatures. Thus, while our age has brought an enormous general advance in knowledge and understanding, the agents of that advance often suffer as individuals from cultural impoverishment. We would be much better scholars, teachers and persons if we wrote less and read more—if, that is, we read the great authors in English and other literatures, and much outside literature proper, without any ulterior motive. When we do such reading it is too often because we are looking for something in connection with something else. The literary commonwealth would be healthier if we could now and then fulfill Macaulay's definition of a scholar—a man who reads Plato with his feet on the fender.

The mention of health brings up another question which is not on the assumed agenda for this meeting but may come home to those present more than some of the topics discussed. Administrators may share a belief that departments of literature have a conspicuous tendency to develop internal friction. My happy memories of a long sojourn at the University of Minnesota include the sardonic witticisms of Dean J. M. Thomas, who belonged to the English department; he remarked one day, in a moment of envy: "When a member of the history department names a colleague, he faces the east." If there is a special degree of dissension in literary departments, it may be because many teachers of literature are artists, actual or *manqué*, and artists, we all know, are a temperamental and irritable tribe. Both the creation and the study of literature are matters of individual experience, not of organization and co-operation; interpretations of

art are not harmonized under the benign pressure of government contracts or statistical tables. We may recall our survey of the many approaches to literature, all or almost all of them essential; and that very fact differentiates the study and teaching of literature from most other fields of learning, where differences are likely to be less numerous or less sharply opposed. Members of this or that school of critical theory may be so sure of their own special enlightenment that they have small respect for the dim spark granted to others. In scholarship and criticism it seems that all service does not rank the same with God. I have no cure for the hundred years' war in departments of literature. Let us say that it is the price that has to be paid for a high degree of imaginative talent.

This question prompts one last comment. As I just said, the creation and the assimilation of works of art are matters of individual experience, and they cannot be measured. The worship of commonness, the natural religion of the crowd, has had impressive support from the statistics of sociology and psychology; mediocre uniformity has been not only the norm but the ideal, and the uncommon individual, on whom the quality of civilization depends, has been only a troublesome bug in the computing machine.

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1. *Nature and the Greeks,* Cambridge University Press, 1954, p. 93.

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The Changing Climate of Philosophy

BRAND BLANSHARD

I

Philosophy has undergone a revolution in recent decades, and I have been asked, as one who has lived through it, to give you some account of it. When I entered the University of Michigan as a freshman fifty years ago, there was no warning of what lay ahead. My first teacher of philosophy, Robert Mark Wenley, typified the attitude of the time. He came into philosophy not from science but from the humanities. I do not know whether he had ever done an experiment in a laboratory, but he had read his Greek and Latin classics, he knew his Dante and Goethe, he had Carlyle and Ruskin and Browning in his head and heart, and to a callow undergraduate his lectures were models of grace and erudition. He was much concerned with religion. The echoes of the Darwinian controversy had not yet died away, and philosophy was the natural resort of enquirers who, like myself, had been brought up in religious households but had religious doubts. Wenley was typical further in being unoriginal. He was a Glasgow Scotsman whose philosophy came straight from Edward Caird. Except for James and Santayana at Harvard and Dewey at Columbia, American philosophers were saying little that did not come out of their teaching in Europe, and more particularly in what was then regarded as the haven of philosophy, Germany.

What was that teaching? It was, of course, idealism. Not the simple idealism of Berkeley, though that did enter in, but the full-bodied, august idealism of Hegel, as magisterial as it was obscure. Many American teachers had studied the system in Germany, and the others studied it in the more intelligible versions of such British interpreters as Green, Bradley and Bosanquet. Idealism dominated the departments from coast to coast. It was the philosophy of Royce and Palmer at Harvard, Ladd and Bakewell at Yale, Butler at Columbia, Ormond and Hibben at Princeton, Fullerton at Pennsylvania, Garman at Amherst, Everett at Brown, Creighton at Cornell, Bascom at Wisconsin, Howison at California. These were distinguished names in their day, though mostly forgotten now.

They were cultivated and dedicated minds, a little naive perhaps about science, but convinced that philosophy had a voice—and indeed the last word—in literary criticism, in theology, in morals and in politics. Not that they had any full understanding of their dark master Hegel; that is a boon that is granted to few. But they had caught the spirit of that greatest of all rationalists. Reason for Hegel was not mere logical nimbleness or dexterity in dialectical sword-play; it was an activity of mind that was present in degree in every experience, and in the reasonable mind was in the ascendant completely, permeating its feeling and controlling its will as well as appointing its convictions. For the absolute idealists philosophy was far more than an academic subject; in a sense it was not a subject at all: philosophy was "thinking about everything else philosophically." What started as a free elective in the sophomore year was supposed to pass over, as it actually did in some of Palmer's students at Harvard and Meiklejohn's at Amherst, into a habit of reflectiveness, a prejudice against prejudices, an identification of one's self-respect with one's reasonableness, which was a permanent transforming yeast in one's mind and character. Owen Wister's little book *Philosophy Four* is a testimony to the wisdom diffused from Palmer's classroom. One can hardly lay it to mere accident that from the Balliol College of Jowett, Green and Caird, a college that never reached half the size of the present Haverford or Swarthmore, there issued a line of statesmen, from Cromer and Curzon to Grey and Asquith, that changed the temper and raised the tone of British public life.

The philosophy dominant in those days has all but vanished. Whereas in 1910 nearly every philosophic chair of importance in Britain or America was occupied by an idealist, not a single chair in England, only one or two in Scotland and a minute number in the United States are still held by men of that persuasion. What has taken its place?

If that means "What philosophy is now dominant in academic Britain and America?" the answer is, the philosophy of analysis. If it means, "What is dominant on the continent?" the answer is, existentialism. If it means "What is dominant in the world?" the answer is, these two plus Marxism. I take it that our primary interest is in the English-speaking countries, only secondarily in Europe and not at all, for the moment, in what is going on behind the curtain. I shall therefore give chief attention to the philosophy of analysis, with a much briefer notice of existentialism and none to the theoretic outlook of communism.

II

Analytic philosophy does not take quite the same form in any two of its exponents, but it is safe to say that the trunk from which all its leaves and branches have sprouted is logical positivism or empiricism. The roots of this vigorous growth go all the way down to Hume, but its first modern shoots appeared in Vienna. The now famous Vienna Circle began to hold meetings under the leadership of Moritz Schlick in the twenties. Its members, personally very different, had intellectually much in common. First, every one of them was a scientist; none of them had been trained originally as a philosopher. Secondly, they felt a joint aversion for the kind of philosophy that had long prevailed in Germany, the sort of metaphysical speculation that enabled Professor X at Freiburg to make Sinaitic pronouncements which were contradicted by the authoritarian thunderings of Professor Y at Jena. Thirdly, and not unnaturally, they felt that the cure for such chaos upon Olympus was the introduction into philosophy of scientific method. Scientists did not contradict each other in this anarchic way, for among scientists there was an accepted method of settling their differences; they could put their theories to the test of experience. To be sure, there was room for difference of opinion as to what scientific method meant, but here the members were helped by their joint admiration for the great physicist from whom their own name for the circle was taken: they called themselves the "Verein Ernst Mach." Ernst Mach was a physicist who thought that even in physics there was far too much philosophy, and who tried to make it a thoroughly empirical science by squeezing the speculative water out of it. He thought the scientist was talking irresponsibly if he accepted anything whatever that could not be presented in sense perception, and he accordingly refused even to accept the existence of atoms except as X's which, inserted in his equations, helped to bring them out right, but which denoted nothing in the physical world. For Mach the laws of nature were rules connecting actual or possible percepts with each other.

In this conception of Mach's lay the seed of one of the chief contributions of logical positivism. As I see it, there were three such contributions —three theses any one of which would be of profound importance philosophically, and which, taken together, were revolutionary. These three positions are the verifiability theory of meaning, the analytic theory of a priori knowledge, and the emotive theory of ethics. They sound technical, and when developed by exponents who would rather write in logical

symbols than in German or English, they can be very elusive indeed. As a matter of fact, they are quite simple. This became suddenly apparent when the Vienna doctrine was transported to England and received translation at the hands of its chief English advocate, A. J. Ayer, now professor of logic at Oxford. His lucid little book on *Language, Truth and Logic* is probably the most influential philosophical work that has appeared in English in the last thirty years. When he published the first edition in 1936 at the age of twenty-six, it was widely read with a sense of outrage, and he was regarded as the *enfant terrible* of British philosophy. It is a measure of the speed with which philosophy has been changing that when he was called back to Oxford two years ago as professor of logic, he was regarded by many as the most conservative of the Oxford professors in his field.

III

Let us look at the three main points in the positivist outlook. First there is the verifiability theory of meaning. As formulated by Schlick, this was a principle that applied not only to philosophy but to all statements whatever that asserted about matters of fact; it was that the meaning of any such statement was its mode of verification. This formula had a double use: it enabled one to find out whether a statement had any meaning at all, and if it did, what that meaning was. Suppose one says that God is omnipotent and omniscient; the principle asks us: "Just where would you look in experience for the properties answering to 'omnipotent' and 'omniscient'? Would your experience differ in any way if your statement were true from what it would be if the statement were false?" If you cannot point to any such difference, your statement must be set down, not as false, for it has no meaning to be either false or true, but as merely meaningless. And this suggests that if it *is* true, what it means is something that is or might be given in experience. If you say there is a Sphinx in Egypt, what you mean is what you would see if you went there. If you say there is an elephant in the room, your statement is meaningful because an elephant is the sort of thing that can be seen and felt, but it is false because you see or feel nothing of the sort.

To many students of philosophy, this teaching opened the door of a hothouse where they had been living and breathing with difficulty. It cleared the air. It assured them that if they had found many of the ideas of philosophers unintelligible, that might be not because they were obtuse but because the philosophers were obscure or worse. Reichenbach seized

on some statements of Hegel about reason being a substance and held them up to indignant ridicule. Carnap seized on Heidegger's sage pronouncement that nothing nothings, and snorted at it reverberatingly. Since there was no possible way of verifying such statements, they meant precisely zero. The positivists went about this work of puncturing windy pomposities with an iconoclastic gusto. They now had a test of both meaningfulness and clearness, in the light of which much that had passed as philosophy no longer made sense. Strangely enough, they gained very little support in their own countries, in spite of the old Teutonic tradition of murky pontification, and scored a great success in Britain, where there was already a fine tradition of lucidity stretching from Hobbes to Moore. Nevertheless this abrupt demand upon philosophers that they stand and deliver did set speculators in Britain and America to re-inspecting their own ideas.

When they did so, the results were disquieting. Metaphysics, which they had taken to be at the heart of philosophy, was now placed off limits for all competent practitioners, and the stock profundities with which metaphysicians had opened the eyes of the groundlings wide with awe no longer had even the dignity of falsehood: they were technically and literally nonsense. Plato had talked about abstract ideas as if they were real things; Aristotle had talked about matter as something that existed without form; Spinoza thought that causes logically necessitated their effects; Kant thought that he was a self distinct from his changing feelings and ideas; Hegel thought that space and time as we know them are unreal; Bradley held that the world is an absolute mind. Ask yourself, said the positivist, how any of these statements is to be verified in sense perception, and you will see that it is impossible, for the reason that there is no meaning there that could be so verified; they are all well-meaning fakes. And while metaphysics is being ushered out, it will save trouble if we dismiss its aged and ailing cousin theology also. Theologians talk about a first cause and an end of the world; they assume that such phrases as "the Trinity," "the incarnation," "miracles," "original sin," "the soul," "transubstantiation," "redemption," refer to facts. Where are the facts that would serve to verify any of these ideas? If they cannot be adduced, the ideas lack meaning. They too must be dismissed.

To all this it was objected that it would test the speculations of the philosopher and the dogmas of the theologian in the same way as the hypotheses of science. To which it was replied: "Exactly—and why not?" As Lord Russell put it, "What is knowledge is science, and what is not

science is not knowledge." There seems to be no province of the world of fact that has not been appropriated by one or other of the sciences, and if philosophy is to provide new knowledge, it must therefore compete with science. This, said the positivists, is an impossible enterprise, which could only end with the discrediting of philosophy altogether. Must philosophy, then, simply close up shop? Not quite, they answered. To be sure, philosophy must abjure the search for knowledge of the world and turn that over to science; it must content itself with being an adjutant to science. What sort of work is left to it? Its work, according to Ayer, Carnap and Wittgenstein, will now be the clarification of terms. Both science and common sense are constantly using words of great importance with no clear sense of their "cash value" in experience—such terms as "matter," "mind," "life," "I," "cause." The philosopher's new and humbler office was, with the aid of his new theory of meaning, to examine these terms—to reject them if they did not refer to anything verifiable and to define them precisely if they did. Wittgenstein held that the business of philosophy henceforward was with "the logic of science."

By the new rule, then, the philosopher was forbidden to say anything he could not verify. But the rule soon turned out to be most ambiguous. Am I forbidden to say anything that I cannot *in fact* verify? In that case, I cannot say with meaning that there are mountains on the other side of the moon, and of course I can. The rule was then read to mean that I must not say anything that I could not *in principle* verify. But in that case I cannot say that you have a toothache, since I cannot even in principle verify your having sensations. For a while, therefore, Carnap and Ayer maintained that whenever I spoke about your feelings and ideas, I must be speaking about your bodily behavior; their new behaviorism was called physicalism. This, to their credit, they soon abandoned, and adopted in its place the thesis that only those statements are meaningful that could in principle be verified by *someone;* your feelings could now be talked about because they could be verified by you. But this again was ambiguous. Did it mean verified conclusively? In that case, I am speaking without meaning when I say that if Caesar had not crossed the Rubicon, he would not have been assassinated, or that anything exists when it is unobserved, or that there are photons and electrons; no one can directly or conclusively verify any of these things. Since this would hardly do, the position was revised to read that only those statements are meaningful for which some empirical evidence could be offered. But then the whole of metaphysics came rushing back again. For there is no statement about

God or the absolute or time or eternity for which some relevant evidence cannot be offered. The movement toward excluding speculation came full circle, and readmitted it in wholesale fashion.

The attempt to push metaphysics out of philosophy by means of a test of meaningfulness thus failed; no formula for the test was ever agreed upon by the new empiricists. Yet the attempt did have its effects. Philosophers became aware that looking over their shoulders as they wrote were a group of acute and exacting critics who would call them to account for every term, who would greet obscurity with a groan and pontification with a yawn. Philosophy lowered its key and made its style more strictly prose; uncertain that God, freedom and immortality had any meaning any longer, it wrote copiously on the meaning of counter-factual conditionals and what could be meant by saying "the king of France is bald" if there were in fact no king of France.

IV

The wings of philosophers were further clipped by the second thesis of the positivists, a thesis with a technical sound but with sweeping implications, namely that a priori propositions are statements not about things but about our meanings or verbal usages. To see what this implies for philosophy, we must remind ourselves of the part that reason has played in its past. Philosophy as we know it is an invention of the Greeks; you will recall Whitehead's remark that the history of Western thought is a series of footnotes to Plato. The Greeks were lovers of abstract reasoning, and Plato is said to have inscribed over the door of his Academy the inscription: "Let no one who has not mastered geometry enter here." The great philosophers who belong to the Greek succession, such as St. Thomas, Spinoza, Hegel, McTaggart, were rationalists who thought that they could sit down in their studies and arrive by reasoning at a knowledge of the ultimate nature of the world. McTaggart, for example, in the two volumes of *The Nature of Existence* finds it necessary only twice to settle any point by reference to experience—once to determine whether anything exists at all, and again to determine whether what exists is one or many. Given these two bits of empirical information, he can spin out the rest of his system by self-evident steps of reasoning. All rationalist thinkers have depended on such reasoning even where they have given a greater place to experience. And such reasoning is a priori: it is not like the inference that swans will be white because we have always found

them white. It is *necessary* reasoning, like that of mathematics; we can see that its conclusions *must* follow. Two and two do not merely make four in fact: you can see that they must do so; the suggestion that they make three or five is unthinkable and impossible.

Since the great systems of the past were constructed by means of such reasoning, the second thesis of the positivists seemed to supply them a scythe with which all rationalists could be mowed down with a sweep. The thesis was that no necessary proposition tells us anything about the world of fact: it only reports what we mean by certain words. Two and two make four. Yes, undoubtedly, but why are we so sure? Because we are talking about what we mean; we are saying that by the word "four" we mean the same thing as by the phrase "two and two." We say that a straight line is the shortest line between two points. Is this an insight into nature? Not at all. What it says is that being the shortest line is part of what we mean by "straight," that to any line that was not the shortest between its ends we should refuse the name of straight. A priori statements thus say nothing about facts: they tell us how we use, or propose to use, words.

The proof of this is that if they were about facts, facts could refute them, whereas if a fact is offered in disproof of an a priori statement, we merely laugh at and refuse to accept the refutation. Somebody says that two and two don't necessarily make four because when you add two drops of quicksilver to two others, you have one drop, and when you add two lambs to two lions, two and two make two—two lions. You brush this aside as irrelevant because you know that whatever may happen causally when two pairs are added, you still mean by the "two and two" just what you mean by "four," and that no event in nature can affect that in the least.

This makes clear what positivists mean when they call such statements analytic and conventional. They are analytic because they tell you nothing new; they merely analyze what you mean. Here the positivists came down squarely on the side of Hume as against Kant. Kant's greatest triumph over Hume and the empiricists lay, he thought, in showing that not all knowledge came from experience, because some knowledge could be gained by sitting down and thinking, for example the whole of mathematics. Such thinking was not analytic merely, in the sense that what we said about a subject set out what was already contained in the subject; it was synthetic because it added something fresh to our knowledge. This the positivists deny. Bertrand Russell says that after completing with

Whitehead the magnificent volumes of *Principia Mathematica* he went on a walking expedition in Norway with his pupil Wittgenstein, on which he was convinced by Wittgenstein that mathematics was only a vast tautology, whereupon his interest in mathematics began to fade. He has pointed out, too, that much of his greatest work was not done by thinking in the ordinary sense, which would have been a dangerous intrusion, but by the almost mechanical manipulation of symbols according to rule. That suggests why so much of the more complicated calculations of modern military and business logistics are turned over to machines, which can do them more rapidly and accurately than any mind.

We may now see also why a priori statements are called conventional. They are like definitions, since they state how we use words, and we may define words as we please. The initial propositions of mathematics and logic are no longer considered self-evident, because self-evident propositions are compulsory; they are called, rather, primitive propositions, and we can make new ones if we wish. Among the postulates of Euclid, for example, we can insert a substitute for the parallel postulate, and instead of saying that through a given point outside a straight line we can draw only one parallel line, may say that we can draw many such lines or none; we can then go on to develop geometries just as consistent as those of Euclid. Even our logic is arbitrary. We can define implication in many different ways, and by so doing we may develop alternative logics, all of them equally consistent.

Now if all this is true, philosophers of the past have been laboring under a vast illusion. They have prided themselves on their logic, the closeness of their reasoning, because they were convinced that logic gave them the bony structure of the world, and that in the degree to which they could render their thinking logically necessary they were conforming to the external and eternal frame of things. They now see that not only logic, but deductive thinking generally, condemns itself to factual unimportance by reason of its very necessity. For necessary statements report only thought, not things, only our rules for the use of words, not laws that hold the universe together. The great and gloomy truth has at last dawned upon philosophers that so far as their thought is rational or necessary it is not revelatory, and so far as it is revelatory it is not in the full sense rational.

It is worth noting that on this central point the positivists have carried most analytic philosophers with them. In professional circles nowadays, to volunteer the remark that a priori assertions are synthetic or state fact

is to place oneself among those gallant survivors of the Edwardian age whose loyalty to outworn splendors is touching but for livelier minds an anachronism.

I am not here to intrude opinions, but perhaps I ought to confess by way of warning that here I am among the troglodytes. To admit that statements are necessary, which means that there is no alternative to accepting them, and then to add that they are conventional, meaning that there *are* alternatives to them, is not particularly convincing. When positivists argue their case, they give no sign of taking the logic they use in that case as something arbitrary and open to rejection. Nor do I think that, because the law of contradiction says nothing about anything in particular, it therefore says nothing at all; logic and arithmetic are not the less true of fact because true of all fact. And so far as I can see, there still remain many propositions like "whatever is colored is extended" that no ingenuity has succeeded in showing to be anything but necessary, synthetic, non-conventional and informative about fact. Unhappily, I must leave it at that.

V

Now for the third thesis of positivism. This thesis has produced almost as great a revolution in ethics as the other two theses have in the theory of knowledge. Ethics has had a strange history in the past half-century. It started off with a surprising amount of agreement. The most influential teachers in the leading countries, such as Moore and Rashdall in England, Paulsen in Germany, Ribot in France, Palmer and Everett in America, all taught that right conduct lay in producing the best results in the way of intrinsically valuable experiences, and though they differed mildly as to what made an experience valuable, they had no doubt that these experiences could be weighed against each other and reasonable judgments made about conduct. There were practical difficulties, of course, in predicting the results of our conduct, particularly upon other minds, but this did not affect the all-important truth that in any situation there was some action which, of all those open to us, would produce most good and would therefore be right, while all other actions would be wrong. This meant that moral judgments, however hard it was to be sure about them, were as objectively true or false as anything in science, that there were objective standards in morals, and that these standards were independent of what people happened to think or feel.

Then two things happened. The first was a tremendous development of

anthropology, stimulated by pioneer works like Frazer's *The Golden Bough* and Sumner's *Folkways* but continuing through Westermarck and Malinowski and Boas to Ruth Benedict and Margaret Mead. Sociology departments burgeoned in all the universities; and in the second world war many thousands of young Americans became amateur anthropologists themselves, observing alien folkways from Paris to the South Seas. The main conclusion from all this, a conclusion that seemed tolerant, sophisticated and liberating, was that what was right in Middletown was not right in New Guinea, and neither could afford to dictate to the other. Morality was relative (blessed word). Sumner's dictum, shocking when he made it, became almost a truism: "the mores can make anything right." And if this was true, the notion that there was an objective standard, that anything was really right as opposed to socially approved, must be discarded.

The anthropologists found an unexpected ally in the positivists, who were not as a rule much interested in ethics, but found themselves forced to a like conclusion by their theory of knowledge. If any statement is to be meaningful, they held, it must be either a statement of fact which can be verified by observation, or a necessary statement about our own meaning. Now a statement that an action is right is neither the one nor the other. It is not an empirical statement, because rightness is not a quality one can observe, like square or red; nor is it a necessary statement, for then to say that promise-keeping was right would be to say that it was right necessarily and always, which we clearly do not mean. But if a statement must be either empirical or a priori, and the statement that something is right is neither, the inevitable conclusion is that it is not a statement at all. This conclusion the positivists drew. Moral judgments were not judgments at all, for they expressed nothing true or false. But they surely expressed something; they were not simply meaningless; and what they expressed, the positivists said, adapting a useful phrase of I. A. Richards, was *emotive* meaning. They expressed, not the speaker's belief about an action, but his attitude of attraction or repulsion toward it. The theory was therefore described, perhaps by way of booing it, as the boo-hurrah theory of ethics. And it arrayed on the side of ethical relativity, though in slightly different forms, some of the most acute and advanced of philosophic minds—Russell and Ayer in Britain, Carnap and Reichenbach in the United States.

In the past twenty years this theory has been the chief storm-center in ethics. It has been attacked as destroying the basis of any objective

decision as between conflicting men or nations, as reducing morals to anarchy, as justifying anything that eccentric moral tastes may approve. Lord Russell has confessed that he does not find his own theory satisfactory, but adds that he finds other theories even less so. And to refute an ethical analysis by saying that it leads to unethical results is not likely to be convincing: it must be met on its own grounds by showing that it has analyzed our moral judgments incorrectly. I think this theory can be shown to be unsound without any impugning of motives. It implies, for example, that when we make moral judgments about the past, e.g., that Brutus did wrong in killing Caesar, we mean to say nothing about the character of his act when it occurred but only to express our own repugnance to it now. This is clearly contrary to our intention. It implies, again, that an undetected murder toward which nobody takes an attitude has nothing wrong about it. There is something deeply amiss about such a theory.

Looking back now on the three theses of positivism, where was philosophy left for those who accepted them? Ethics in the old sense had ceased to exist, for talk about the right, or the good, or the *summum bonum* was talk without cognitive meaning, and a discussion of people's attitudes toward conduct was not ethics but psychology or sociology. Metaphysics had ceased to exist, for if what it said was verifiable in perception, this belonged to one of the sciences, and if it were not so verifiable, it was a pseudo-proposition passing itself off as meaningful. Theology had ceased to exist, for the Deity was not observable. Was anything at all left of philosophy? Yes, logic was left, though it was now to be a study of the rules for the transformation of sentences. On the whole, however, the positivist conclusion was very close to the sweeping negation reached two centuries earlier by Hume. "If we take in hand," said Hume, "any volume, of divinity or metaphysics for instance, let us ask: Does it contain any reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion."

To be sure, positivists did not deny that philosophy had any function at all; their most articulate spokesman, A. J. Ayer, now professor of logic at Oxford, held that its business was to offer definitions. Such terms as "matter," "mind," "self," "cause" plainly had some meaning, and the appropriate work for the philosophers was to sort out the empirical wheat from the metaphysical tares in such meanings and formulate it as clearly as he could.

VI

We see here in germ a new development in philosophy which since then has been much in the public eye—the linguistic philosophy now associated with Oxford. This philosophy had two chief sources, both of them as it happened, in Cambridge—G. E. Moore and his successor in his Cambridge chair, Ludwig Wittgenstein. Moore was a sturdy defender of common sense in philosophy. In an essay of 1926 he offered a long series of statements that philosophers had made about the unreality of time, the non-existence of matter, etc., and insisted that by his unaided common sense he could see that they were all false; he was far more sure of his common sense beliefs than of the soundness of the dialectics that sought to invalidate them. This being true, he suggested that the business of philosophers was not to flout common sense, as they have so long delighted in doing, but to analyze its beliefs and clarify them. You may quite well know that a belief is true, he said, without knowing what its correct analysis is; you know, for example, that knowledge is good, though you might find it a desperate business to say precisely what either "good" or "knowledge" means. The main business of philosophy, then, will be the analysis of what we mean by the basic terms of common speech. Here Moore was close to Socrates. You will remember that the Platonic dialogues are largely concerned with taking such common words as "virtue," "courage" and "piety" and creeping slowly up on their meaning by a process of intellectual trial and error till a definition is reached that will stand. Moore also conceived the business of philosophy as the analysis of meanings, and by concentrating his scalpel-like intelligence for many years on such meanings as those of the verb "perceive" and the adjective "good," he raised philosophy to a new level of minute and self-critical precision. To be sure, he never knew to the end what analysis was, and said so with characteristic candor. If you came up at the end of the process with a concept different from what you were supposed to be analyzing, could this be an analysis of *it* at all? If you did not, and merely formulated the ordinary meaning, was this of any importance?

One school of linguistic philosophers insisted that it was, and that by failing to remember this, philosophers had gone wrong. Professor Malcolm of Cornell wrote: "The philosophizing of most of the more important philosophers has consisted in their more or less subtly repudiating ordinary language." When such language refers to situations known at first hand, it neither is nor can be wrong; "ordinary language," said

Malcolm, "is correct language." Hence when Bradley announced that time was unreal (though admitting that it took time to write down the sentence), and Russell said that, since the causal theory of perception was true, he could see only what was in his own head (though continuing to talk about seeing mail-boxes and motor cars), there was no reason to take them seriously. Their theories were in plain conflict with ordinary usage, which is correct usage; what they were really doing was proposing that the words "time" and "see" should be used in a quite new way, and a silly, way. The new linguistic school even insisted that Moore's defense of common sense should be read as a defense of common language; that what he was really saying was that only by sticking to common language can we avoid being led astray. Moore repudiated his own followers on this point, and Russell, who has been from the beginning an influential figure among the analysts, emptied upon the ordinary-language philosophers many vials of acidulous wrath. If we accepted the meanings of ordinary language, he pointed out, we should have to abandon physics. When the ordinary man says he sees the sun, he clearly means that he sees the sun as it now is, whereas this is always false: he is only seeing the sun as it was eight minutes ago. Indeed we never see anything in the physical world, though the plain man thinks we do. Ordinary language is full of such distortions of fact. As Berkeley said, if we speak with the vulgar, this should not prevent our thinking with the learned.

This sort of linguistic philosophy is, I think, on the wane. But since the war a more subtle variety of language philosophy has been developed at Oxford from cues provided by Wittgenstein. Philosophy has a central part in the curriculum of both ancient and modern "Greats" at Oxford, and with its forty or fifty able philosophers, Oxford is the most powerful single influence in the philosophic world. It was therefore of historic importance when Wittgenstein moved into the vicinity of Oxford and made personal contacts with some of its leading philosophers. The brilliant and witty John Austin, whose lectures used to be announced under the heading "Sense and Sensibilia, by J. Austin," was probably the most influential of them, but he died a year ago, and Gilbert Ryle, editor of *Mind*, is now the leading spirit. Ryle's main point is that the problems of philosophy have grown out of a misunderstanding of what he calls the logic of words. The plain man can say such things as "Satan does not exist" or "the world began" without being confused; the trouble begins when the philosopher gets to work on them. Under the influence of grammatical analogies, he has come to think of all proper names as refer-

ring to substances and all predicates to attributes; and of course if that is true, "Satan" stands for a substance and "exist" for an attribute. But so to read them is mere chaos, for Satan then becomes, as for Meinong, a kind of existing non-existent, and existence itself a queer attribute, which makes no difference to the character of what has it. Philosophers, knowing that to say "the rain began at four" makes sense, think it makes sense to say the world began; Vice-Chancellor Lightfoot of Cambridge was only mistaken in *fact* when he said the world began on Friday, 23 October 4004 B.C., at 9:00 a.m. But he was really contradicting himself. For while rain can begin in time, time itself cannot begin in time; that would mean that it began at 9:00 rather than 8:59, though by hypothesis there was no 8:59. Ryle maintains that philosophic problems generally have grown out of such "category mistakes" as he calls them; and he holds that the business of philosophy is simply to clear these up by more careful attention to the logic of words. Logic for Ryle does not mean formal logic merely: it means also the study of what words will go with what; and there are as many different categories to recognize as there are words or groups of words that could conceivably fight with one another in a sentence. Philosophy is the topography of these categories; that is, it is the attempt to lay out the relations of these words or phrases with each other in such a way as to avoid clashes in logical grammar.

The cry has gone up nearly everywhere that this sort of thing is trivializing philosophy. The charge is not quite true. Ryle is really talking metaphysics while trying to avoid it. Why can't we say that time began or that Socrates is a number? The words don't sensibly go together, to be sure, but the only way we can see this is by seeing that the things they refer to won't go together—by seeing, for example, that a man and a number being what they are, one could not possibly be the other. There is nothing verbal about that insight. And yet it has been a widespread fashion for some years to approach problems of philosophy as if they were really linguistic problems and to entitle solid books about them—*Ethics and Language, The Language of Morals, The Language of Politics, Logic and Language*. Such titles naturally repel the reader who is interested in things rather than in ways of talking about them. The new fashion has tended to divert attention from substantive to verbal questions by refusing to distinguish the two. To say that the problems of philosophy generally arise from linguistic confusion or can be settled by linguistic study—the problem, for example, of what is right and wrong, or whether there is a God, or whether we shall survive death, or whether things exist

when no one perceives them—seems to me hardly more than a tiresome fad which time may be trusted to deal with. It is a fad that Oxford, with its wealth of classical and historical knowledge, can perhaps afford to support, but the danger is that if it should become general, philosophy may die of boredom.

VII

I have dealt at some length with analytical philosophy because in this country it is far and away the most important of recent philosophical developments. There is one other, however, that must have at least passing mention. This is existentialism. If we were talking about philosophy on the continent of Europe, this would be our main theme. The great figures in Germany in recent decades have been Heidegger and Jaspers; in France, Jean-Paul Sartre. Existentialists and analysts stand at opposite poles from each other, and I was interested to note at the two last world congresses of philosophy—at Brussels in 1953 and Venice in 1958—that the two schools could hardly make contact at all: the Europeans thought the British trivial and hair-splitting; the British thought the continent isolated in a metaphysical pea-soup fog. The Americans were, on the whole, on the British side. Existentialism has made so little headway in American academic circles that I shall confine myself to two comments about it: one a remark about what it is, and the other a note on its invasion of theology.

The thesis of existentialism is the cryptic one that existence is prior to essence. It is really a declaration of war on rationalism and indeed on systematic philosophy generally. The existentialists abhor such philosophy because they think it skates on the surface of things; it deals in concepts or abstractions; when it talks about men, it means man-as-such, a shadowy simulacrum that Plato disastrously supposed to be more real than any individual man. The rationalist philosopher thinks that the world consists of an enormous web of such concepts, tied together by necessity, and that if he could get this webwork clearly before him, he would have a complete understanding of reality. This the existentialist thinks is illusion. What such a system leaves out is existence. A man is not a lump of abstract characters, for an imaginary man might have these just as well: the point is that he exists. That is a point that mere intellect cannot deal with. Intellect can single out and see connections between characters that are already given, but it cannot explain why they should be given; it cannot explain why there is anything, the world for example, rather than

nothing at all. Intellect can deal only with essences; existence is not an essence; therefore intellect stands helpless before it.

From this the existentialists draw some striking conclusions. First, since concepts or essences are linked together by necessity, the person whose thought is dominated by these connections or who lives by principle is a slave. If he is to free himself from such slavery he must fall back on that in himself which lies below all essences, his existence. Since this existence does not consist of characters, and what does not consist of characters is for the intellect nothing, his real self is nothing. Being nothing in the way of content or character, it cannot be governed by necessity and is therefore free. Freedom consists in the self-affirmation of this naked self, its commitments in the way of non-rational choices or explosive acts of will. Secondly, since these are undetermined, and the fate of the self unpredictable, life is at every move precarious. We have been somehow pitched into a world we did not make and cannot hope to understand, a world that for Heidegger and Sartre is a godless world. The free self stands over against this world as something different in kind from it, not at home in it and estranged from it, yet facing a destruction as capricious as its birth. In such a world the appropriate attitude is one of *Angst* or anxiety, as Heidegger would say, or nausea, as Sartre would say. All that is left us is the courage of despair. We must live from moment to moment, responsible not to principle or to mass sentiment, which strive to put us in straitjackets, but to ourselves alone.

That such a view should find acceptance among the French and Germans of post-war days is not perhaps surprising. When everything was overthrown that they had lived by—pride in their nation, belief in human justice and goodness, belief in the power of reason to control conduct or to achieve agreement—a desperate nihilism was natural; existentialism is a philosophy of each man for himself. That such a philosophy, or rather anti-philosophy, should satisfy minds at peace with themselves and others is scarcely credible. Because it has lost the anchor of reason, it may drift in any direction; it approved for Heidegger a flirtation with Nazism that even his countrymen find hard to accept, for Sartre a rather noisy communism, and for Marcel a mystical Catholicism. In a sense it is secure from refutation, for if you abjure reason and merely act, there is nothing to refute. Its starting point, as someone has said, is not *cogito ergo sum* but *volo ergo sum*—I will, therefore I am. But in many dark volumes existentialists have tried by reasoning to persuade us that reason is a false guide. If they are to succeed in this, they must pay more regard to an

unformulated law of reasoning that has been called the law of excluded muddle. To anyone accustomed to the sharp precision of the analysts, existentialist reading is sheer penance.

VIII

It is among the theologians, where Christian charity reigns, not among the philosophers, that the existentialist voice has been heard. The voice heard in these circles is not the atheistic living voice of Heidegger or Sartre, but echoes of a peculiarly passionate voice silent now for more than a century, that of the strange and melancholy Dane, Søren Kierkegaard. The echoes have been transmitted in a muffled way through the monumental works of Karl Barth, Emil Brunner and their American colleagues, who if such popular magazines as *Time* and *Life* are called in evidence, have a far wider hearing than any mere philosopher. The reason for the resurrection of Kierkegaard, I am inclined to think, lies in a certain parallel between his day and ours. In his day the peril to Christian orthodoxy lay in the rationalism of Hegel; today it lies in the rationalism of science. Kierkegaard devised a way to meet Hegel which, when examined, has proved a promising way also to meet scientific rationalism. This way is, in essence, to deny that religion is an affair of reason at all, that it stands in need of scientific support or is open to philosophical criticism. According to this new, or renovated, theology, man was created free and pure, but by misusing his freedom he introduced into human nature a taint that has clouded all his faculties—his conscience as well as his reason. There is no longer therefore a road from man to God, only a road from God to man. Man's blighted human reason is utterly unable to construe God's nature or will, and even when Deity does reveal himself, it is by a descent of grace, a kindling of faith, a Divine imperative that is a sheer miracle, inexplicable to our psychology and discontinuous with our logic and our ethics. The attempt to explain this by human reason is sinful pride of intellect, for the appointed field for reason is nature, and it is helpless before the supernatural. Revelation need not be, as it is for Catholic thinkers, consistent in the end with reason; God's ways are not our ways; he is, as Kierkegaard once said, the most absurd of all beings, for, unamenable to our logic, his nature combines attributes that to us are inconsistent, and unbound by our ethics, he can with perfect virtue command a father to kill his son. As for the father, his not to reason why, his but to do, and to let his son and his own morals die.

This sharp break between religion and ethics on the one hand and the domain of reason on the other seems to have achieved great popularity in our theological schools, and it is perhaps not difficult to see why. By an apparently simple distinction it puts the content of faith once for all beyond the reach of intellectual criticism. The faithful can now watch the rising tide of science without that sense of threatening encroachment felt by the religious liberal; like St. Augustine, they are citizens of a city that is in this world but not of it, and not subject to its laws or fate. They can face all rationalists, like the Sphinx, with a fixed inscrutable smile.

One must sympathize, of course, with much in this high adventure of hope and faith. But one may well remember, too, that it has been tried before, and not with wholly reassuring results. The trouble is not merely that, as McTaggart reminds us, no one ever tried to break logic but what logic broke him: it is that this theological existentialism, like its secular counterpart, is morally ambiguous. Throw the compass of reason overboard, and on the exalted instrument that remains to us the needle shifts distressingly; the "divine imperative" seems uncertain of its pole, veering now to inspiration, now to fanaticism. This is not of course the first time that reason has had to stand trial. Indeed this very issue was argued a century ago, with Dean Mansel—a better writer and a clearer thinker than Kierkegaard—appearing for the prosecution, and John Stuart Mill appearing for the defense. Mill's case, though brief, was so devastating that Mansel has been virtually dead ever since. No Mill has yet appeared to lay the lively ghost from Denmark. But he will. And one can anticipate the judgment that it would have been better if the devoted Walter Lowrie had allowed Kierkegaard to rest in peace.

IX

When you asked some of us to review the present situation in our subjects, you asked us as workers in the vineyard, not as administrators. But we cannot forget that we are speaking to administrators who have practical responsibility for the teaching in our colleges, and I hope you will not think I am going too far beyond my brief if I venture to draw some conclusions of my own from this view of recent philosophy. They are offered, of course, as no more than the opinions of one observer of very limited vision.

First, a department dominated by either one of the main schools of current thought, the analysts or the existentialists, is educationally defi-

cient. Analysis, particularly linguistic analysis, is too thin a diet; as Whitehead said, if men cannot live by bread alone, still less can they live by disinfectants. Students have a way of scenting out subjects of importance and granting to professors of the unimportant the leisure they desire; a department consisting of analysts will probably sit in some corner of the campus discussing its puzzles with a small coterie of specialists and count for little in the intellectual life of the community. An analyst has plaintively raised the question whether, after all, importance is as important as people think it is, and it must be admitted that for a philosopher to disparage clearness and precision of thought would be something like treason to his calling. But he can at least say with Kant that concepts without percepts are empty, that not all distinctions are worth making, and that a knife is meant for cutting, not for being honed.

On the other hand, a department of existentialists is like a chorus of wails issuing from a dark Druidic wood. They sound a strange note that makes one anxious—about what, one is not quite clear—and earnest about a total commitment—if only one could discover to what. I would no more decry the importance of being earnest than of being clear-headed, but if such existentialist literature as I have read is typical, it falls notably short as discipline. We want our students to be able to write clearly and think straight, and these things the existentialists cannot teach because they seem never to have learned them. They may be profound and prophetic; they may be able to produce Delphic pronouncements on call from their cavernous subjectivity; but after all, the Delphic oracles were commonly enigmas, and philosophy exists to banish enigmas.

The ideal philosophy department, particularly if it has graduate students, will represent no one school. Such students will profit greatly by exposure in turn to a positivist, a Thomist, a Deweyite and a Whiteheadian, and the more competent and convinced each of these is, the better. It is true that this process of multiple exposure may confuse the weaker students, but such students should not in any case be encouraged to teach in this field; and for the stronger students it is an invaluable apprenticeship in intellectual self-reliance.

Secondly, a word about the sequence of courses. The best introduction, I think, is a course in problems of philosophy, which gives the student some notion of the range of issues he may explore, and of their exciting possibilities. These issues are best stated in the words of the philosophers themselves; excellent anthologies and paperbacks are available.

After forty years of teaching Berkeley, I still delight in the simplicity and power of his arguments for the mind-dependence of what we see and feel, and these arguments should come to the student as a revelation of what one can achieve by mere thinking, without specialized or technical knowledge. In most colleges, apparently, philosophy begins in the sophomore year. But there is something to be said for opening the introductory course to freshmen; they have had nothing like it in high school, and it makes them feel from the outset that college opens new and wider horizons. At Yale philosophy at all stages is elective. We are at present offering the problems course in each semester; and that it answers a genuine need and interest is suggested by a present enrolment of over 400, including 150 freshmen. The best second course, I think, is ethics, with which some degree of specialization begins, but in a field where everyone has had experience. Logic should be required of all majors, but should be open to any student without prerequisite. Such a course should not consist primarily of symbolic logic; if it does, those who are good at mathematics may find it boringly easy, while the many who hate mathematics may be merely repelled by it. In some institutions the history of philosophy is used as an introduction. This, I think, is a mistake, since so many historic theories answer questions that the students have never raised and as yet feel no need to answer, so that the course seems like a parade of arbitrary notions. Of the many other courses that can be profitably offered, I would mention the philosophy of science as one that is destined for an increasingly important part among liberal studies.

Thirdly, a remark or two about methods of teaching. Philosophy is of all subjects the one best suited to teaching by discussion; although, as Plato said, it is a dialogue of the soul with itself, the dialogue is much easier to sustain if another soul sits opposite. It is likely to gravitate to the central place wherever discussion is the staple method of instruction, as in the tutorial system at Oxford, the directed studies program at Yale and the honors system at Swarthmore. I used to think the system at Swarthmore, with seminars numbering four to six and lasting about three hours, had its advantages even over the Oxford tutorial; I am afraid that I at least should have been numbed into speechlessness by a Mark Hopkins at the other end of the log. But small seminars, while pedagogically ideal, are expensive, and the question is thus bound to arise whether philosophy can be effectively taught by the lecture method. The answer, I suggest, is that lectures of the ordinary type are of very little use; on this point I recom-

mend Henry Sidgwick's *Lecture Against Lecturing:* why should a student listen to a loose and loosely expressed discourse when he could read a good book on the matter instead? The lecture seems to me defensible, though then it is more than defensible, only if it is in itself a model performance—only if it provides through the lucidity of its argument, its firmness of order and the felicity of its statement, an inviting example of the intellectual attack on a problem. I have known lecturers from whom one went away saying to oneself: "Wouldn't it be wonderful to have that easy a mastery of one's intellectual resources?" If students are overheard saying that, their lecturer is a success. Unfortunately this is a kind of success that is hard won and often comes late. Men who are two or three years out from their doctorates are probably still talking the pedantic language of their dissertations: it may take twenty years and a ripe maturity to achieve genuine perspective and simplicity; and it is to these mellower minds that the exacting business of elementary lecturing should be committed.

Fourthly, does anything follow from what has been said about the place of philosophy among liberal studies? If philosophy is only a defining of terms, or a study of ordinary usage, or an attempt by reason to deal with the irrational and ineffable, then its place is humble indeed. And it may be that the days of its reign as queen of the sciences are over and that it will have to accept instead that office of handmaid to the sciences which the positivists would assign it. Once when a Yale chair of philosophy fell vacant, the eminent William Graham Sumner rose in faculty meeting and proposed that Yale take the lead among universities by abolishing the department of philosophy altogether; science dealt with facts, philosophy did not, and among the sciences it was therefore a fraud. Unfortunately a dispute arose forthwith as to what a fact was, a dispute in which philosophy had to be called in to state the case for its own abolition. This seems to me symbolic. Those current philosophies that would limit the work of philosophy to some narrow and menial office always support their thesis by an appeal to philosophy in a wider sense. What is this inevitable wider philosophy?

It consists for the most part of two things—analysis and synthesis. Analytic philosophy is an examination of the concepts and assumptions of ordinary life. Many key terms of common speech that we use unreflectively and glibly—life, mind, self, cause, time, truth, good, ought, God—are dark with mystery when we try to get clear about them, and such

trying *is* analytic philosophy. Again, our most universal assumptions—that things are what they seem to be and remain the same when we are away; that we can argue from past to future; that what we shall do a moment from now is as yet unsettled; that there is a right and wrong independent of our prejudices—all these and many more are seen to be open questions the instant a critical eye is fixed on them. These are central fundamental questions; they are not adequately covered by any of the natural sciences; they are a further part of analytic philosophy.

This form of philosophy is in a sense a prolegomenon to science, an examination of concepts and assumptions which the scientist uses but does not examine. Synthetic philosophy goes beyond this, and comes after science rather than before it. Each natural science is an attempt to answer one large question; physics asks: What are the laws of matter in motion?—biology: What structures and laws of behavior are exhibited by living things?—and so on. The cautious scientist does not enjoy making raids across his own borders. But sciences are like nations; they need a United Nations and a secretary general if they are to be at peace with each other. The most challenging problems of synthetic philosophy are those that break out *between* the disciplines. Is natural science right that every event has a cause, or is traditional ethics right that to say this about choices would destroy responsibility? That is the problem of free will.Is physics right that all motions are the product of other motions, or is psychology right that my intention makes a difference to the moving of my lips? That is the problem of body and mind. The synthetic philosopher—Leibniz, Hegel, Spencer, Alexander—is the sort of philosopher who takes the pieces offered him by science and common experience and tries to put them together into some consistent picture. Here again is a work that must be done, indeed that must be done over again in each generation as knowledge advances.

X

Philosophy so conceived is plainly of the first intellectual importance. But granting this, is it of practical importance as a liberal arts discipline? On this large topic I have time for only two concluding remarks. First, a subject is presumably practical if one finds it frequently and helpfully applicable. I venture to think that philosophy is in this sense highly practical, not as a set of conclusions but as that habit, that "unusually stubborn effort to think clearly," which James said *was* philosophy. Other

disciplines may be described in this way too, above all perhaps mathematics. But philosophy seems to me a more useful discipline than mathematics because it reproduces more of the conditions of ordinary thinking. In mathematics your concepts are provided for you, already sharply defined; and your conclusions are either necessary or worthless. In philosophy clearness has to be won in matter that is at once familiar and nebulous; to get clear as to what one means by "liberty," "duty," "truth," is to discipline oneself in crystallizing fog into definite manageable shapes. That is what one must do if one is to think effectively in politics, in religion or in the law. Whether students of philosophy end up with the conclusions of their professor is not of great importance. What is of importance is that they should carry away an acquaintance with philosophic method and a respect for the philosophic mind. "It is important," said Edward Caird, the philosopher and Master of Balliol, "that a proposition be reasoned, and it is important that it should be true, but it is more important that it be reasoned than that it be true." If it was reasoned and mistaken, it carried its remedy within itself, while if it was true but unreasoned, it was true by chance and helpless against attack. Philosophy as the habit of critical reflection is the most practical thing one can carry away from a liberal arts education.

As the years go on, such discipline becomes more essential. Knowledge is growing far faster than the power of the mind to assimilate it. One must specialize if one is to be more than a sciolist anywhere, and as the specialists become more specialized, they have less in common. With the present stress on science, there is a danger that the old common coin of educated men, which made it possible for them to discuss morals and art and literature and religion with mutual understanding, should disappear from the currency. Here philosophy can help. Its problems are not those of any special science but of all men. Its greater writings are literature that does not belong to any one time; Plato, like Homer, has been read with delight in every intervening age. The method of philosophy needs no laboratory and no instruments, for ideally it is just the way of right reason as such, applicable alike to every subject. What divides men is prejudice and practical interest: the more they think, the more likely are common principles to appear; there is no Russian logic or Chinese multiplication table or, rightly speaking, any German or American philosophy. Reason is the same, and followed honestly, leads in the same direction, in all men.

As philosophy helps us to keep our lines open with humanity, so also

it may help to keep us afloat on the tidal wave of modern knowledge. It is the business of the synoptic philosopher to see things whole if he can; he is not averse to detail, but the detail must have a significance beyond itself. It is of course not philosophers only that show the philosophic mind. Newton studied the falling apple, Darwin his beetles and butterflies, and Freud slips of the tongue philosophically, because they studied these things in the light of their bearings on principles of vast sweep. Not all knowledge is worth having: of the making of many books there is no end; and, as Jim Brosnan, the big-league pitcher, has sagely remarked, "There's nothing like a book to keep your mind from thinking." "People often think that the more one knows, the better," said Tolstoy. "This is untrue. The point is, not to know much, but to know that which is most necessary of all that can be known." The art of reading, said Walter Raleigh, is the art of judicious skipping. But skipping in the intellectual world must be on some principle. To select what is important from the ceaseless parade of facts and ideas that our books and journals marshal before us calls for standards, and standards call for reflection, and such reflection is philosophy. Against the partisan slogans, the Pollyanna faiths, the self-righteous nationalisms of our time, the nightmare art, the beatnik poetry, the erotic stage, the violent television, the perpetual adolescence of the screen, there is perhaps no full protection. But the best I know is that habit of detached and critical reflection which belongs to the philosophic spirit. To expose our students to the contagion of that spirit so effectively that they catch it, whether from the pages of Plato or Mill or from our own halting example, is to bestow on them a gift beyond price.

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Recent Developments in Economics

BEN W. LEWIS

Economics is concerned with the ways in which societies use their limited resources to satisfy their unlimited wants. As it enters the sixties, it has quite a job on its hands.

Within recent years our own society has experienced a depression of unprecedented severity and a war of unprecedented proportions and consequences. For the fifteen years just past we have been mobilized in a cold war of technology, materials and nerves. The pressures and clamor of our interdependent world grow steadily more insistent and strident, and the business of living and making a living together becomes more complex and baffling by the hour. We have been and continue to be involved in harassing problems and momentous decisions, both substantive and structural, respecting the use of our means and our manpower. It would be strange if the world we have known for the last third of a century—depression, war, inflation, foreign aid, big business, farm surpluses, big labor, growth, unemployment, television, atomic breakthroughs, defense, budgets, taxes, gold flows—had not pressed its mark deeply upon the development of economic thought and practice, and upon the thinking and activities of economists.

It must not be supposed, of course, that recent development in economic thought has not in fact been *development*—that a "new economics" with no link to the past arose full blown from the ruins of the stock market crash of 'twenty-nine—or that external forces alone have fashioned the new face which present-day economics is wearing. Events and the problems they have reflected and generated have given timeliness and special character to these changes in economic thought, and have emphasized their usefulness. The roots of the new economics, however, are imbedded firmly in the past. Felt needs have lent urgency and provided propitious occasions, but the changes were, in any event, scheduled for delivery in due course.

It is true, nonetheless, that recent years have witnessed unusual activity and imaginative drive in economic thought. Economists have been on the

move in areas both of ideas and of the application of ideas to pressing political-economic and industry-economic problems. Origins and influences aside, economics today has a bright look and a jaunty stride. Its business is still, as always, with society's basic economic problem—the managing of scarce resources—but its current preoccupation is with aspects of the economic problem which for decades it was content to take for granted. In addition, it is cultivating even the older and well-worked fields with newly invigorated intensity, by new methods and with new tools. It is enlisting the aid of sister disciplines in re-examining and testing its assumptions and its "laws": mathematics and psychology have joined up in strength, and others, notably engineering and administration, are feeling the urge. Electronic computing machines are becoming standard equipment; today any unmechanized economics department stands naked in the presence of the natural sciences. Economics is moving, in swelling volume, beyond the study, the classroom and the laboratory into government and industry. Economic counsellors are becoming symbols of status in the offices of men of affairs. Confidentially, we shall probably have an economist in orbit—collecting sun-spot data for cycle theory—before the year is out.

Let us begin our discussion of developments in economics by asking, in the best professional tradition: "Developments about what?" What *is* economics? What do economists do—between elections?

Economics as a social science is concerned with the ways in which society economizes—the ways in which, as a people, we manage our productive human and natural resources and the goods and services which derive from their employment and use. Economics starts from the simple, undeniable proposition that, over all, mankind does not have and never will have enough human, natural and produced resources to enable all men to have all they want (and will come to want) of goods and services. Mankind's desires for goods in the aggregate are so vast that no purpose is served by regarding them as other than *unlimited:* the resources available to society for the production of goods in the aggregate are clearly *limited.* In relation to our total desires, our resources are scarce. We cannot visualize a time when all men will have more of everything in the way of private goods and public goods than they want. Certainly no such affluence is known today, anywhere.

The fact of scarce resources poses *the economic problem* that faces all societies and dominates all economic situations and issues. It also poses the task of economics. Since our limited means, even at their very best and

most productive, will not permit all of us to have all the goods we want, we are forced to be concerned about how our means are used—how fully they are used, how efficiently they are used, what goods they are used to produce and in what quantities, and in what proportions the goods are distributed among us all—and, of course, what goods must be forgone. The degree and manner and direction of resource use and the disposition of the product of resource use have, of sheer necessity, been of primary moment to all societies through the ages. And all societies have established and maintained (or acquiesced in) economic systems or *economies* —man-made and man-remade arrangements to turn out and effect the decisions that society wants to give to the multitude of questions generated continuously by the basic economic problem.

An economy—more accurately a political economy—as it exists at any time, and irrespective of the extent to which it is market-oriented or government-oriented, represents society's wishes at the time, relative to the conduct of its members in their use of resources and to the arrangements for articulating its wishes and putting them into effect.

The discipline of economics is the study of the economic problem in all its parts and manifestations, and of the arrangements, institutions and processes which men have devised to grind out the answers to the manifold questions which it raises. It is the job of economists to gather facts, to identify, to analyze, to interpret, to establish causes and consequences, to predict and, at least as citizens if not as scientists—citizens at home with the issues and the conditions—to appraise and to recommend.

What, then, has been happening recently in economics, and what have economists, as economists, been doing? Parenthetically, let me say that this is precisely the right moment for stock-taking and inventory by college administrators. After 20 January, 1961, campuses are likely to see very few economists—principally between 'planes.

"Recent" is a relative term, and I suspect that in use its span varies directly with the vintage of the user. Most economists, however, would find it easy to establish a close connection between today's happenings in economics and events in the 1930's, and particularly to trace today's methodological and substantive ferment to something which a man named Keynes dropped into the compound at that time. Others too were adding pinches. The smell of yeast was in the air.

Economics in the thirties was ripe for a Keynes. For decades, economic "theory" had concentrated on refinements of economic value and the shares of income going to the functional participants in the production

of goods. Economic value was accepted as an expression of underlying economic forces operating logically and with certainty in a self-contained, harmonious market system, and hence as a guide to the proper economic conduct of individuals, acting freely in their own self-interest and constrained only by competition, in their dual capacities as producers and consumers.

Society expressed its wishes for goods and services, not politically, but through the offers and bids of individuals in the market; and the processes of the market, responding to and reflecting those wishes, produced the pattern of resource use and division of income that society regarded as best. The free, impersonal market, peopled by competing, self-seeking individuals, was the classical interpretation of society's answer to the economic problem. This was the system; "price" (economic value) was the touchstone, the "unseen hand" which rewarded human effort and directed the flow of society's human and natural resources and the production and consumption of goods.

It probably goes without saying that the social philosophy implicit in this notion of a "natural" economy was, for a long period, entirely congenial to the prevailing political temper. Not that everything was going smoothly in the economic world—there was, indeed, much in the way of poverty and economic insecurity and injustice—but in considerable measure these unfortunate happenings were looked upon as aberrations, and as exceptions which proved the rule. They were discords which would presently be put in tune by the inherent tendency of the system toward a happy, harmonious equilibrium. Economists, with the exception of a few noisy dissidents and reformers, were immersing themselves in the task of adding elegance to the systematic structure of value and distribution theory. Economics offered relatively little on public economic policy beyond indicating by its whole bearing that society would do well to confine its political economic activities to a little patching and caulking on Saturday afternoons.

Public economic problems multiplied, however, in the years following World War I. Cyclical business instability, want and insecurity, industrial and financial concentration and manipulation, market excesses, farm surpluses, terms and conditions of work—all of these and more were beating on the door for active attention. The economic world grew more and more complex, and the discordant voices among economists became more insistent and assumed a tone of greater reason and authority. The preoccupation with systematic theory grew less; concern with new approaches

and currents—historical, statistical, institutional, evolutionary—grew greater, as did sheer concern for problems of human welfare. Increasingly, economists came forward with pronouncements on monetary policy, on anticyclical measures, on antitrust and public regulation. Even systematic theory was restless, but as yet it had little to say to a restless world. Its impressive dwelling place was beautifully furnished with antiques, but unfortunately the heating, lighting and plumbing also were antiquated.

Then came the long, bitter winter of the Great Depression; and the New Deal and the economists were not far behind. Economics, as distinct from economists, was slightly delayed.

Things really began to pop for economists in the spring of 1933. The economic dikes were leaking, and economists with eager thumbs were standing in line. The rush of economists to Washington was joined by many pseudo-economists; it was not always easy to tell the difference. Panaceas and nostrums abounded, but in the special areas in which economists had been doing some sustained, realistic fact-gathering and homework, genuine progress was made; and in other areas, where improvisations were piled upon improvisations, economists in their more sober moments did much to modify and turn to good account some of the excesses generated by the spirit of the time. In any event, with or without —or despite—the ministrations of economists, much that was long overdue in social, developmental and regulatory reform was enacted into law and became part of our political economy.

Neither economics nor economists, however, were prepared to deal deftly and surely with the over-all paralysis of business and industry and the mass unemployment that struck, and threatened the existence of, the American economy in the fall of 1929. Much theoretical work had been done on the nature and causes of business instability, and increasingly the analyses were being fortified by statistical investigation and interpretation. Nevertheless, we entered the thirties with an apparatus of theory which by and large concentrated its attention on problems of adjustment within an assumed framework of full employment. Its target was an harmonious equilibrium in the specific use of economic resources: that the resources would be fully used save in exceptional circumstances was taken for granted by most—not all—economists.

Keynes and a growing number of others did not take it for granted, and since 1936, when Keynes' *General Theory of Employment, Interest and Money* reached the profession, most economists have come to agree,

by diverse paths and with varying degrees of conviction, that any equilibrating forces at work within the free market economy in its pristine state are entirely likely to leave the economy in a state of rest at a level of activity substantially short of full employment. They also agree that something can be done about it.

The economy was sick in the thirties, and strict classical economics offered little sympathy and no help. Its stock remedy was "the wringer" —the system had committed excesses; it must purge itself. Individual economists turned their talents to diagnosis and prescription, but their efforts were scattered and, except to divert the patient, were without effect. It remained for the *General Theory* to supply a systematic explanation of the condition and a systematic basis for what has now come to be our accepted public approach in dealing with it. The argument was simple and, against the background of traditional economics, bold: the forces that make for full and useful employment of our resources are known; unfortunately they do not operate mechanically, and the complex of individual decisions in a free market economy cannot be counted upon to keep them strongly and steadily in motion. The economy, behaving *naturally*, is quite likely to slow down in its output of goods and payment of incomes, and when this happens, its *natural* processes of recuperation operate, at best, too slowly and uncertainly for a desperately anxious world to contemplate with equanimity. The "unseen hand" is more adept at steering than at starting. Conscious collective action through government is required to provide directly or to encourage and stimulate the amount of spending necessary to maintain production and income continuously on a high level.

The long-time concern of economics with the precise working and internal adjustment of the several parts of the economy was now joined by an equal (for a time, indeed, by a greater) concern with the over-all performance of the total economy looked upon as a vast production-income engine. The processes of the engine were subjected to searching analysis, and the forces affecting the fullness of its performance were isolated, identified and arranged systematically for study.

Old concepts, attitudes and approaches that had long held sway in theory and policy underwent an agonizing reappraisal. It was realized that both the flexibility of wage rates and the elasticity of the demand for labor had been overestimated, and that the usefulness of falling wages as an automatic restorer of employment in the event of a widespread slackened demand for labor was substantially less than had been supposed. The

faith of the twenties in monetary policy as a preventive or corrective of serious depressions gave way, perhaps because it was not sufficiently manifested in aggressive action, to (possibly excessive) disbelief: under depression conditions an expanded supply of money would not produce a commensurate increase in spending, and with great excess capacity already in existence, no achievable reduction in interest rates could induce enough new investment to bring the economy out of its slump. It was recognized that saving could be carried too far: over-saving, through the resulting lessened aggregate demand for goods, could impair the prospects for profit, and hence slow down the whole production-employment-investment process. A new concept—the "multiplier"—was developed and was linked usefully to an earlier concept—the "accelerator"—in analysis of the nature and effect of investment expenditure, and the importance of investment as a positive, driving force in the maintenance of the economy at a high level of productive operation was finally given full recognition.

A significant change in fiscal theory and policy—also bearing the mark of Keynes—got under way.

Traditionally, "sound" government finance required annually balanced budgets, the use of taxes solely to raise revenue to cover expenditures, and expenditure programs dictated solely by their substantive qualities. It was known that large budget deficits could be inflationary, but there had been no inclination to employ budgets, taxes and expenditures to regulate price levels or to affect national output and employment. The first budget deficits in the thirties arose from exigency rather than theoretical design—the responsible forces were simply the sharp automatic decline of revenues, resulting from the drop in national income, and the difficulty of reducing expenditures without undue sacrifice of accepted programs. The major increases in government expenditures at this time were directed to the immediate alleviation of distressing problems rather than to the achievement, by the application of carefully formulated and well developed theory, of a massive turnabout in the unemployment situation.

But with the development of the Keynesian and Keynesian-like theoretical framework came the positive employment—with a "sound" rationale—of fiscal policy to promote economic stabilization. Even the time-honored canon that a "good" tax should have a steady yield went by the board, to be supplanted by the belief that a "good" tax should be highly sensitive to income changes. National debt, long regarded as a burden

loaded by a profligate government upon the backs of a suffering citizenry, was cast in a new role as a useful and respected instrument for controlling the structure of interest rates, private investment and consumption and, hence, for affecting the level of employment.

Led by Keynes, aggregative economics appeared on the scene in strength. Macro-economics moved in; micro-economics moved over. A strong groundwork of theory was laid for positive full-scale governmental action to maintain stability at a high level of activity in the economy—theory so convincing and compelling that since 1946 (a short ten years after the *General Theory*) it has been entirely respectable for the government to grasp the "invisible hand" in a firm and completely visible grip. The Employment Act of 1946, accepted by both political parties in Congress, proclaimed the duty of the government to use its full powers to stabilize economic activity at a high level, and established the Council of Economic Advisers to keep the President and Congress informed on the state of the economy and to recommend measures and programs to carry out the government's responsibility. In the years that have followed, huge governmental expenditures dictated by other than strictly economic considerations have made the task of preserving full employment less pressing; but both President Truman and President Eisenhower acknowledged their faith, and kept the government's fiscal and monetary weapons constantly in readiness, and upon occasion used them with a light touch to combat both incipient recession and inflation. They are being readied now, in the presence of developing recession, for early and probably heavier use by the new President.

Not the least important of the developments for which Keynes is entitled to a measure of credit occurred in methodology. National accounts, as searching and revealing in their way as the income and other financial, income and balance-sheet statements of private corporate enterprises, were constructed to throw light on society's economic enterprise—its total economy—and a flow of pertinent data from significant sectors of the economy was instituted on a large and growing scale. Analysis came increasingly to be suggested and supported by statistics. New areas of investigation beckoned, and throughout economics it became increasingly possible, through the use of newly acquired data, to clothe bare hypotheses and speculative assumptions with facts. Great impetus was given—and with great effect—to the search for materials and improved tools for economic analysis, measurement and testing. Economics, economic policy and economic investigation and analysis were on the move.

And so, indeed, was economic literacy. "G.N.P." became, shortly, a household term. It became possible to introduce "deficit financing" into the conversation in refined circles—with some care, of course. The theology of thrift became noticeably less overpowering: little children were permitted to argue with the local banker when he maintained that thrift was one of the cardinal social virtues. And the textbooks! Anyone who worked his way through "Principles of Economics" just before the Great Depression and who waited until the post-Keynes era to reopen an introductory textbook for the first time could know real sympathy for the befuddled suburbanite who wakes up in the wrong house—the covers are the same, but the interior is full of surprises. Beginning students used to trace the automatic adjustments in the economy; since Keynes they have been turning the economy on and off.

The full impact and significance of Keynes and his early associates cannot yet be measured and assessed. This is not the occasion to offer a pronouncement on how much of what is called "Keynesian Economics" is properly to be credited to other (earlier and contemporary) thinkers, or precisely how much of Keynes (clearly, not all) was valid and will remain as accepted economics. For a quarter of a century, economists—first-rate, recognized economists—have been analyzing, interpreting, qualifying, extending, attacking, rejecting, defending, accepting Keynes. Every level of economics and virtually all areas have been affected. Whether or not the *General Theory* was a masterpiece, neither economics nor economists have been the same since it appeared.

Keynesian economics was not the only bounty afforded by the turbulent thirties: still another major development in economic thought—this time in the worked-over area of micro-economics—broke the surface, a development which was to make a deep impression on the body of economics studied at all levels, and also on the research activities of economists. The study of value and price (the heart of classical economics) had proceeded, for well over a century, largely through analysis of market models in which the competitive component was assumed to be of distilled purity and compelling force. In discussions, brief acknowledgment was paid to monopolistic markets as a special case. Economists realized, of course, that completely competitive and completely monopolistic markets were abstractions from reality, that the markets of the real world were rarely ruled either by competition so inflexible and powerful in its compulsions as to permit no policy discretion to sellers, or by the uncontrolled whims of a single seller. The typical approach, however,

was to deal with the "laws" of value in terms of the two paper extremes (it was tidier that way), and to look upon the wide range of real market situations lying between the poles as a dark forest of frictions, deviations and exceptions—off bounds to serious economists.

In the thirties the vast shadowland was explored by a few venturesome souls and opened for settlement. Today this is where most micro-economists reside. The decade saw the beginning of sustained attempts to evolve systematic theories of value and price relevant to the market situations where virtually all of the world's buying and selling transactions actually take place, markets characterized by the presence of both competition and monopoly in varying degrees and for a variety of causes —markets in which sellers are few ("duopoly" and "oligopoly") and markets in which, although the number of sellers of similar goods is large, the goods offered by each seller are different from those offered by other sellers in ways deemed significant by buyers ("imperfect competition" or "monopolistic competition"). The concept of "perfect competition" which, less carefully delineated, had long been the principal prop of value theory, was trimmed, sharpened and appointed to serve as a special analytical tool.

The essence of the new approach consisted not in a sudden revelation of the real nature of markets, but in the growing conviction that for too long economic theory had sacrificed reality and utility to refinement. The front parlor was spick and span, but in the kitchen nothing was cooking.

The movement, once under way, expanded rapidly and flourished. New concepts (e.g., "workable competition") appeared in course; new and extremely useful analytical tools (e.g., "marginal revenue") were devised, and important phenomena (e.g., merchandising, advertising) which had long intruded and as long been shunted into footnotes were embraced and integrated into emerging theory. Real (oligopolistic and monopolistically competitive) markets and market structures became the scene and the stuff of promising institutional and quantitative investigation and systematic analysis. Not all of the promise has materialized, but results continue to be significant both for public and business policy and for sheer insight into things as they are; and even the realized limits of the movement have served to suggest new directions for investigation with new (interdisciplinary) tools and procedures.

Related matters, distinguishable (although not always carefully distinguished in economic discussion) from the phenomena directly involved

in the new approach to markets and price, also came under penetrating study in this period—notably corporate concentration and the divorce of corporation control from corporation ownership. This had important implications for, although it did not succeed in achieving definitive conclusions on, such troublesome items as profits, entrepreneurship, motivation, price behavior, organization and capital formation and investment. It also sparked interest in and action on public policies bearing on business organization and capital and product markets.

In discussing the influences on developments both in macro-economics and in micro-economics, it should not be overlooked that public economic issues were calling for analysis directed to real business, industry and market situations; and that in the thirties, for the first time, through the good offices of the New Deal, economists were being exposed *en masse* to these phenomena in the raw. History may not be too kind in its final appraisal of the N.R.A. and certain other New Deal extravaganzas, but for what they did to educate hundreds of callow American economists fresh from their classrooms, and for the precedent they established in the use of economists in the public service, even the sternest history must concede them at least a laudatory footnote. The Great Depression and the New Deal did not alone produce the new developments in economics, but they certainly provided a rich soil, a congenial climate and some of the seeds.

The movements in economics that got under way in the thirties gained impetus from World War II and picked up momentum in the years that have followed. The war brought economists in droves again to Washington and to government posts throughout the country, and again the call was for the use of positive economics in the making of public decisions. The allocation of strategic materials, the pricing and rationing of goods at all market levels, the pricing of labor services, the direction of labor effort and of investment, the export and import of commodities—all of these tasks and more which, under what we somewhat fancifully refer to as "normal" conditions, we entrust largely to the processes of the market, were made the subject of conscious public determination and action. This was positive economics on a grand scale, and the whole complex afforded a magnificent model for the testing of economic hypotheses across the board. Conditions did not permit the testing to be carried out with scientific precision, of course, but economists learned much and were impelled to learn more. You can learn quite a lot about a railroad—or an economy—by trying to run one.

Marked advances were made during the years immediately preceding World War II in our understanding of the great problems of macroeconomics. Since the War the advances have continued, but it is evident from the post-war experience of our economy and its economists with inflation, unemployment, development and growth that the end of the assignment still lies ahead. We have been confronted by a persistent rise in the general level of prices and at the same time have been nagged by unemployment. Our normal interest, stemming from simple considerations of human welfare, in the factors and processes favorable to long-run improvement in the performance of our own and other economies has been recharged by international political developments. Economics is at work in all of these areas, at levels both of theory and policy, and with all of the hypotheses, instruments and procedures it can command.

Monetary theory, in partial eclipse for a number of years, is presently enjoying a considerable revival. The venerable Quantity Theory of Money, accepted in some form by most economists before the Great Depression, suffered severe reverses with the coming of Keynes, and was virtually swept from the stage. The earlier interest in the price level or purchasing power of money was superseded by a growing concern over income theory and the behavior of real output and employment. The lines of the quantity equation ($MV=PT$) gave way to the lines of the income equation ($C+I=C+S$), and in some versions of income theory, the supply of money played a very inconsequential part. Since the war, however, the quantity of money has managed a comeback as an important variable, and at the moment is sharing the stage, in a strong supporting role, with income theory and fiscal policy. At least two major works in this area have appeared since Keynes, and new syntheses of income and monetary theory are being developed. These are considerably more complex than either of the older and simpler quantity or income theories, and involve a significant modification of both. They embrace the money supply, the demand for money balances, propensities to consume and save, the marginal efficiency of capital, and monetary institutions. Not the least of the forces influencing the nature, direction and extent of the present resurgence of activity in this field are the great flood of statistical material and the development of new and refined statistical methods. These have resulted in greatly expanded empirical and econometric research.

An interesting interplay between theory and policies in the monetary field occurred during and for a short period following World War II.

Wartime experience demonstrated clearly that, even if monetary policy could not end a slump, it could, if directed to the purpose, prevent interest rates from rising despite an unprecedented volume of government borrowing and a consequently unprecedented government debt. This was an enticing discovery to those who found low interest rates desirable —including the Secretary of the Treasury—and monetary policy continued to be devoted to this end even though it tended to conflict with the anti-inflation efforts of other agencies of the government, until an (uneasy) accord was reached in 1951. Only then was monetary policy again directed toward stabilization of the price level. The debate again got under way as to the efficacy of monetary policy as a stabilization tool and as to its proper role in relation to fiscal policy in this capacity.

Controversy within the profession is far from stilled on the relative culpability of cost-push and demand-pull as generators of inflation, and on the relative merits of, and exact assignments to be given to, monetary, debt management and fiscal measures, and to a lesser extent, direct controls, as instruments for its containment. There is lack of agreement as well over the precise shape and composition of each of these sets of instruments, and over the degree and extent to which each should be employed and the precise timing of its employment.

Much the same kind of general observations can be made on the progress of economics in laying open and treating the endemic problem of unemployment. Involuntary idleness, swelling and receding but always present, is the curse of a market economy, both for those directly affected and for the rest of society. We have never been free from unemployment; and since the war, and even now when our economy is performing at record or near-record levels of over-all productivity, we are painfully aware that too large a percentage of those who want to work can find no work to do. Economists are chipping away at the hard facts of this problem with analytical tools that have been markedly refined and sharpened in recent years. There is much agreement on the identity, nature and operation of, and the relationship between, important variables, and a considerable degree of precision and certainty is slowly taking the place of guesses and estimates.

Here as in the case of inflation there still are many unknowns that need to be known, and there will always be unknowns that are unknowable. The operating units with which economics deals are free, independent human beings. Expectations, which play a major part in determining the level of economic activity, can never be captured and secured as surely as

events. Both inflation and employment are played upon by other than economic forces, and in some measure at least, their treatment involves society in a choice between incompatible goals: it is by no means certain that society can have all that it would like in the way of full employment save at the cost of more than it wants of inflation, just as its pursuit of technological advance can be prosecuted fully only at the cost of increased insecurity. All of this is simply to point out that really substantial development in economics is not the equivalent of an end to all economic problems.

Controversies over the diagnoses and the most efficacious prescriptions for the treatment of the mass problems of our economy—inflation, unemployment, growth—as is the nature of controversies, are open for all to see and hear. And, since they are manifestations of the process by which understanding is reached, they are not particularly to be deplored. But what is possibly less evident (and also encouraging), is that the areas of agreement are increasing in size and number. Irreconcilable conflicts over particular theories and policies are giving way noticeably to efforts to devise formulae which will join and so enlist the positive qualities of erstwhile alternative findings and proposals in combined attacks upon the problems which plague us. Not the least important of the propositions on which substantial agreement is in prospect is that the problems of aggregative economics are amenable to purposeful action by an intelligent and informed society. But much remains to be done; the imperative demand upon our theory for greater and greater precision has still to be met. It seems altogether doubtful that economists or economist-coached public officials will ever achieve in the world of affairs the proficiency enjoyed by our introductory students in the classroom in controlling the intricate mechanisms of the economy.

It is tempting to treat the work of economists since World War II in the area of economic growth and development as the counterpart in scope and significance of their activities in the area of income and employment which were touched off by the Great Depression. Certainly there is a similarity in the fact that the increased scientific interest in growth and development is a direct product of the times—in this case the national drive to build up and hold the underdeveloped countries of the world within our sphere of influence, and the national concern over the outcome of our productivity race with the Soviet Union. "Growthmanship" is a national preoccupation, and economists are among those most preoccupied.

Courses in economic growth and development are now standard offerings for both undergraduates and graduates in our departments of economics, and college and university institutes and programs on growth and development, often specially endowed or supported by special grants, are proliferating at a rate matched only by the rate at which articles and textbooks on the subject are pouring from the presses. The already sizable collection of country studies prepared by independent scholars and by teams of experts on the basis of on-the-spot investigation is growing steadily. Foreign students come to us by the hundred to learn the mysteries and acquire the touch. American economists by the dozen, under arrangements with governments (our own and foreign), universities and foundations, are shuttling back and forth between their home stations and the campuses and the development and planning boards of underdeveloped countries, as lecturers and technical advisers. "Become an Economist and See the World" is today a meaningful and alluring slogan. The movement is reminiscent of the mass migration from campuses to Washington in the flush days of the New Deal.

Despite the external similarities, however, it is not yet clear that the current excitement in the area of growth and development is on all fours with the income-employment ferment of the thirties, as measured by its effects to date on systematic theory or on systematic public policy for either underdeveloped or well-developed countries. Much useful analysis, model building and critical debate is taking place, and an impressive array of special growth and development concepts, with a matching vocabulary, continues to emerge. An assortment of theories has been advanced to explain how growth begins and is sustained. There is more than a semblance of agreement on the range of forces and variables that have played a significant part in the past and that probably should be taken into account in constructing development policies and programs to be carried out in the future. There is much less agreement, however, on emphases and phasing. In seeking to learn from the past, it is not always easy (or even possible) to distinguish between causation and coincidence, or to ascertain with certainty the actual direction of causation. The rival claims of chickens and eggs for primacy have nowhere been more baffling than in the area of development and growth.

It is in the study of economic development and growth that the need for the more recently devised methods and techniques of economic analysis (about which more will be said later in this paper) is felt most keenly, and where they have been employed most extensively, and with

a good deal of operational usefulness. Input-output analysis, linear programming and operations research are examples. But more, and more than economics alone can furnish, is required.

One of the most striking qualities of the process by which different economies seem to grow to higher and higher levels of sustainable *per capita* productivity and income is diversity—differences in speed and course, and in the substance and shadings of the total context in which the process occurs. Fruitful study of the many-sided phenomenon of growth calls for skill in the gathering, ordering and interpretation of facts, and for the time-tested tools and ways of thinking of the economist —his concern with order, with scarcity and allocation, priority and opportunity cost, productivity, capital formation, income determination, trade and the like. But the attack on the mysteries of growth, particularly on the forces at work in the early stages of growth in underdeveloped countries, calls also for the contributions of disciplines other than economics. Penetrating and imaginative economic investigation and analysis need to be reinforced by understanding of such factors as motivation, creativity, cultures, taboos, "know-how," organization, social and political institutions and a host of others. Fortunately, all of this is coming to be recognized, and increasingly the insights and skills of the psychologist, the sociologist, the political scientist, the anthropologist and the historian, together with those of the administrator, the engineer and the man of business and finance, are being linked to and integrated with those of the economist.

Admittedly, growth and development theory and policy have not yet "arrived." At the levels of both theory and policy formulation, and with reference to the growth problems present in countries at all stages of economic development, differences are wide and sharp. These differences narrow into crevices, however, in comparison to the chasms that frequently separate development programs as formulated, recommended and "accepted," from programs as put into effect. Virtually every underdeveloped country has achieved or is in the process of achieving (or is having thrust upon it) an impressive "Development Plan"—sometimes in several volumes, with appendices. The "plan" may be followed in practice more or less closely or it may be ignored. In any case, sooner or later, development in greater or lesser degree may occur. One prediction, however, seems safe: an underdeveloped country which exposes itself to, and participates seriously in, the exercise of development planning (even though its planning reflects the present unsettled state of development

theory) will be the gainer—possibly from a resulting "plan"; most certainly from the *process* of planning, from the experience of facing up squarely to the task of economizing.

There is, to be sure, just a touch of showmanship in some of the current development effort—possibly no more than is required to put development theory to work. The main sweep, however, is clearly sound; its sources, directions, procedures and techniques are in the best scientific tradition, and it is making headway. And economists (and airlines) are having a wonderful time.

The classical theory of international trade, also severely shaken by the Keynesians, is currently being subjected to further competitive pressure from new theories of economic growth. Essentially, what was involved in the earlier coup was the replacement of the traditional processes of the specie-flow-price mechanism, as an automatic equilibrator of the balances of payments between nations, by income changes and the attendant multiplier. The gold standard broke down, first in theory (along with the quantity theory of money) and then in practice, when governments refused to permit their internal economies to be ruled by international flows of gold; and the way was cleared for new doctrine and new control devices.

Currently, we are witnessing a most startling, almost incredible phenomenon—probing, critical, damaging questions being directed at the venerable Law of Comparative Advantage, whose standing in the community of economic theory has, until very recently, placed it almost above questioning. True, the law has been brought in for questioning before, but its inquisitors have not succeeded in impairing its reputation. In practice, of course, the law has been disregarded fully as much as it has been observed; on no issue of economic policy have economic scholars over the years been less effective in influencing "practical" policy makers than on the removal of barriers to specialization and trade.

Growth theory has now come forward to challenge anew the theoretical impeccability of the Law of Comparative Advantage. The chief criticism of comparative advantage is that it "is essentially a static concept which ignores a variety of dynamic elements."[1] The issue is important to persons charged with the formulation of development policy governing the allocation of investment funds and scarce resources in underdeveloped countries. "The classical analysis focuses on long-run tendencies and equilibrium conditions, while modern theories of growth [based on assumptions quite different from those underlying the doctrine of com-

parative advantage] are concerned with the interaction among producing and consuming units in a dynamic system." It is suggested that to modify comparative advantage to take account of dynamic considerations would be to "rob the original doctrine of much of its practical value." What is needed, and what is being sought, is a resolution of the two approaches sufficiently specific to be useful in guiding actual allocation decisions. It is by no means clear at this stage that the search will be successful—that any reconciliation will eventuate in principles that can, without modification in individual cases by "other considerations," be employed to dictate development programs.

Great stimulus was given by the war and its aftermath to intensive and imaginative inductive studies of firms, industries and markets. We know vastly more, and we are adding daily to our knowledge and understanding, of the theory and practice of pricing, of market strategies and of investment and output determination, and hence of the real nature of incentives, motives, markets and competition as forces for the ordering of our economy and the achievement of the economic goals to which we aspire. This has direct significance for policy in such fields as antitrust, and also for the rate and service activities of our public regulatory agencies.

Comparable studies and investigations are under way in the economics of labor. Wage negotiations under collective bargaining do not constitute an exact science, but it goes without saying that wages have a tremendous impact upon the economy and all of its participants, and that the formulation of wage standards related functionally to other significant, interrelated economic variables and to declared economic goals is essential if the institution of collective bargaining is to persist as a feature of our free society. The study of wages, in all of their aspects and effects—upon particular and general prices, output and employment—and involving empirical and statistical research and testing as well as model building and theoretical analysis, is making a large claim upon the interest and time of labor economists today.

Space precludes explicit treatment in this paper of related developments in several of the specialized branches of economic analysis and policy which make up a considerable part of any full-blown university economics curriculum—for instance, economics of agriculture, economics of transportation. Most of what has been said in the preceding two paragraphs applies, at least broadly and with much the same force, to the untreated areas. In all fields, modern theoretical work is being related to

observable, measurable variables, and is taking on, increasingly, an empirical, realistic quality.

Possibly the most striking of recent developments is the spate of new methods and tools of economic analysis, and the new language which is being employed in the study and presentation of economics. Mathematics has always been used in economics, but until recently the language of economics was predominantly verbal (as it still is in the introductory courses). Today, however, the benighted economist whose mathematical training stopped with long division finds it quite impossible to read beyond the dedication in advanced treatises, and it is scarcely worth his while to take the wrapper off the current issues of the learned journals. It has become necessary for economics to employ a language that can deal precisely and with logical consistency with the masses of significant economic variables and relationships of which it has become increasingly aware, and which it now takes carefully into account as it moves ahead. Mathematics, to those who understand it and appreciate its possibilities, is such a language. It can, however, be a deceptive language: it can convey the appearance of an exactness which does not, and very possibly cannot, exist; and it has a remarkable capacity to lure the susceptible into mistaking intellectual gymnastics for fruitful research. Nonetheless, the most sceptical non-mathematicians concede that the careful and skillful use of mathematics can extend—and is in fact extending—the frontiers of economics.

The mathematical approach is being supplemented by the *simulation* technique in the construction and use of theories or models for explaining or prescribing economic behavior. Simulation is described as "a technique for building theories that reproduce part or all of the output of a behaving system. The system can be an aggregate of behaving units, an entire economy, or a particular unit, a human decision-maker. The output can be one aggregated element, e.g., that interest rate which clears the money market, or the whole host of thoughts, associations, and actions employed by a man while he solves a specified problem."[2] "The process of simulation involves constructing a theory, or model, of a system that prescribes the system's processes. These processes can refer to macro as well as micro elements and the prescriptive detail reflects the researcher's knowledge of and interest in particular parts of the system. By carrying out the processes postulated in the theory, a hypothetical stream of behavior is generated that can be compared with the stream of behavior of the original system."[3] One writer believes that "The advent of computers

in general and the techniques of simulation in particular open up possibilities for the growth of a new scientific institutional economics."[4]

There has been continued growth in the sheer mass of useful economic statistics, and their refinement and adaptation both to inductive research and to the testing of hypotheses have more than kept pace with the growing volume. Increased statistics are opening new doors for theory, and in turn theory is demanding more statistics. The collection, preparation, analysis and application of statistics have snowballed in the past quarter century, and out of the marriage of statistical analysis and mathematical economics has emerged a thriving "econometrics"—a "type of economic analysis in which the general theoretical approach—often formulated in explicitly mathematical terms—is combined—frequently through the medium of intricate statistical procedures—with empirical measurement of *economic phenomena*."[5]

"By carrying out the different stages of research [the theoretical development and the testing of hypotheses, and even, to some extent, the exploration of the problem] in closer conjunction with each other, a considerably greater degree of economic design can be employed in the research, and the resulting analysis will benefit by greater consistency and integration than would have been possible otherwise."[6]

A considerable volume of decision-oriented economic analysis is going on today under the broad and impressive canopy of *Operations Research* —which has been characterized by a recent expositor as an approach or method or point of view, the essence of which is "that a phenomenon is understood when and only when it has been expressed as a formal, really mechanistic, quantitative model, and that, furthermore, all the phenomena within the purview of science . . . can be so expressed with sufficient persistence and ingenuity." A corollary of this point of view is a "preference for symbolic, as opposed to verbal, modes of expression and reasoning." Operations research, we are told, is "all research [undertaken] in this spirit intended to help solve practical, immediate problems in the fields of business, governmental or military administration or the like."[7] Operations analysts are for the most part engineers, mathematicians, statisticians and natural scientists by training, but several well-known operations research programs are pointed directly toward economic problem situations, and others have important implications for, and may find direct use in helping to resolve economic issues.

Linear Programming seeks to order the combination of scarce resources available to a business firm and to allocate their use as between the firm's

activities so that the objectives of the firm will be achieved in the maximum possible degree—to plot the best course of resource use. It is applicable to a wide variety of "maximizing" situations facing the management of a firm—situations in which conventional micro-economic theory has always assumed that management, because this is the only "rational" way to behave, proceeds deftly, surely and swiftly to optimum choices as between a multitude of competing alternatives. Economic theory knows this assumption to be unreal, but it has not worried too much about the precise nature and extent of its departure from reality. Linear programming, together with research generally in this area, promises to bring greater rationality and higher quality to management decision-making, and in the process to bring a larger measure of reality to economic theory. Micro-economic theory, for example, would be materially enriched by an improved understanding of how price and output decisions are actually made.

Input-Output analysis is allied to linear programming but is concerned with inter-industry relationships and the study of *general* economic equilibrium, as distinct from processes within a firm. It undertakes to calculate the demands which various sectors of the economy place upon other sectors in order to carry out a given program of production, and to reach an harmonious pattern of output levels for the several sectors regarded as a whole.[6] Such analysis is proving useful in the prosecution of development programs, as well as in other situations where public mobilization and allocation of economic resources on a broad front are called for.

Still another promising theoretical development is *Organization Theory*, which recognizes that the individual makes economic (and other) decisions not alone as an individual but within the framework of organizations in which he assumes roles as he carries out the many aspects of his economic life. Organizations, themselves molded by individuals, powerfully affect individuals in their economic activities. The content, shape and limits of organization theory are still fluid, but it is tending to serve as a focal point for game theory, decision theory, information and communication theory, group theory and motivational theory; and model building, mathematics and simulation have all found their way into its methodology. Hopefully, the pursuit of knowledge along these lines and in this context will enhance our understanding of the activities of individuals as producers and consumers, and of firms, markets and, indeed, of whole economies.

A really exhaustive treatment, in extent and depth, of current developments in economic research approaches and methods would be far too exhausting to attempt in this paper. Indeed, it is impossible to account for all of them even by name. At the present stage the lines of development are interwoven, and at some points tangled: in a certain context a given approach is viewed broadly as including one or more other approaches; in a different context the approach finds itself included, possibly with other approaches, as a part of one of its erstwhile components. If the fact that "you can't tell the players (to say nothing of the plays) without a program" evidences a modicum of confusion at the moment, it also indicates the volume of the effort and something of the spirit and energy which characterize current economic research.

Economics has never been livelier. Imaginative programs, each unique but also sharing something with others, are crowding upon each other in a determined drive to explore and explain the complex relationships that lie at the heart of economic life and activity, to bring greater reality and precision to economic hypotheses and reasoning, and to make economics more useful in dealing with the stubborn problems of our times. The very process of devising and fitting out new analytical devices is in itself helping to shed light on the substance of the matters to be analyzed. Anyone who questions that economics today is under the spell of an uncompromising scientific spirit should ponder the implications of the following excerpt from a recent article setting forth research targets in the area of economic decision-making.

"Economics has been moving steadily into new areas where the power of the classical equilibrium model has never been demonstrated, and where its adequacy must be considered anew. Labor economics is such an area, oligopoly or imperfect competition theory another, decision-making under uncertainty a third, and the theory of economic development a fourth. In all of these areas the complexity and instability of his environment becomes a central feature of the choices that economic man faces. To explain his behavior in the face of this complexity, the theory must describe him as something more than a featureless, adaptive organism; it must incorporate at least some description of the processes and mechanisms through which the adaptation takes place."*

Here surely is the crowning scene, the ultimate: tight-fisted, profit-eyed, indispensable "Economic Man" himself, for nearly two centuries the hardiest character in economics, in the clinic for observation, the prognosis unfavorable.

Thus far, we have talked principally about the concerns of economics

—with *economists* behind the curtains. What, specifically, is happening to the guild of economists? Most of them teach, and many of them teach much of the time. There have been developments in the teaching of economics, too, stemming from a rather widespread and growing feeling in the profession that at the college level economists are not getting their message across. We put it on the line, but the students don't dig us.

Economists modestly believe that they have something to say to a troubled economic world; that the economic problems of the world are manageable if they are understood and if they are thought about as an economist thinks about them. But not enough college undergraduates are electing to take work in economics, and too few of those who take economics really grow in understanding. This is not the occasion to parade the private worries and the secret strategies of the profession; let it be noted simply that promising action, both profession-wide (under the sponsorship of the American Economic Association) and by individuals and individual departments, directed to improvement in the *teaching* of economics, is under way. The profession is concerned to make its contribution toward carrying the increased load imposed upon the colleges by the growing avalanche of college students, and particularly concerned to present its offerings in such fashion that they get across and stick and, hopefully, are put to use.

Economists are making still another—and for them a very surprising—move toward spreading economic understanding, by giving active, organized support to improved teaching of economics in the *schools*. The case for economics in the schools is as broad and firmly based as the case for democracy itself. Increasingly, the public problems with which citizens are required to cope are *economic* public problems, and our democratic society will not survive beyond the day when our citizens fail to discharge the political-economic responsibilities which democracy places upon them. Our citizens, for the most part, do not go to college, and the only contact which most of them have with formal economic reasoning is in courses in economics, social studies and American history in the schools. Economics in the schools today is in an unhappy state; just how unhappy is indicated by the fact that the staid, almost immovable American Economic Association has been sufficiently shocked to undertake an active program, in cooperation with educators, to do what it can to repair a situation which its own earlier neglect helped to create.

The ticker is running behind the market, but college and university administrators today do not need to be convinced that economists themselves are a very scarce resource, and that at the current market price the

supply is less than the demand. The forecast is for an upward adjustment in price: even so, rationing is in prospect. The demand curve for economists is shifting markedly to the right as the increased call for their services on the campus is joined by an increased call from business and an urgent new call from government. The flow to business and government has already gained substantial volume, and it will probably reach flood stage in the Potomac watershed in the next few months.

The movement from the campus to business and government—and back—is sound and good. Economists have demonstrated to businessmen and to the government the value of economics and the economist's approach to the problems of business and government. I shall not undertake here to delineate and extol the very special contribution of the economist to policy and practice in these areas; the market speaks on this matter with greater eloquence than I can command.[10] I do venture to assert, however, that as economists return to campuses from the world of affairs (and they *do* return) they are better economists and better teachers, and the economics they prepare and serve is more nourishing and more palatable for their experience.

In the vexing situation which this two-way traffic produces for college and university administrators, may I offer a word of well-intentioned, although admittedly *ex parte* counsel? Extol and support the movement in general: it is good for business, for government, for economics, and good for the colleges and universities. In particular cases involving your own staff, resist gently—show that you care. But, at the moment of decision, acquiesce gracefully in the inevitable. After all, how 're you gonna keep 'em back in the stacks, after they've seen D.C.?

AUTHOR'S NOTE. It will be obvious to anyone acquainted with the range of material covered, however inadequately, in this paper that I am greatly indebted to others for information, ideas and counsel. I have drawn heavily upon *A Survey of Contemporary Economics*, Volumes I and II, edited respectively by Howard Ellis (1948) and Bernard Haley (1952) for the American Economic Association, and upon the more recent series of survey articles in the *American Economic Review*. It is a pleasure to record my special gratitude to Lester Chandler, Bernard Haley, Richard Heflebower, Richard Nelson and Kenneth Strand, among others, for their generous responses to my pleas for help. No responsibility devolves upon anyone but me, of course, for the results.

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1. The quotations in this paragraph are taken from, and the discussion is based on, the manuscript of a survey article, "Comparative Advantage and Development

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2. G. P. E. Clarkson and H. A. Simon, "Simulation of Individual and Group Behavior," *American Economic Review*, December 1960, p. 920

3. *Ibid.*

4. Martin Shubik, "Simulation of the Industry and the Firm," *American Economic Review*, December 1960, p. 917. "Possibly one of the most valuable contributions of simulation to date has been the discipline imposed by the necessity of precisely defining for the computer both the problems and questions to be answered. . . . The discipline called for in making a program that will work is rigorous. The value of this is twofold, both in requiring the checking of models for consistency and completeness and in the designing of consistent data-organization processes." (*Ibid.*, p. 913). See also G. H. Orcutt, "Simulation of Economic Systems," *American Economic Review*, December 1960, p. 893

5. W. Leontief, in *A Survey of Contemporary Economics*, Vol. I, H. S. Ellis, editor, 1948, p. 388

6. Richard Ruggles, in *A Survey of Contemporary Economics*, Vol. II, B. F. Haley, editor, 1952, p. 423. Mention might be made specifically of econometric research directed to the study of the whole economy by employing a dynamic mathematical model. See the review article, C. F. Christ, "Aggregate Econometric Models," *American Economic Review*, June 1956, p. 385

7. Robert Dorfman, "Operations Research," *American Economic Review*, September 1960, pp. 577, 578

8. Robert Dorfman, "Mathematical or 'Linear' Programming," *American Economic Review*, December 1953, pp. 797, 824

9. H. A. Simon, "Theories of Decision-Making in Economics and Behavioral Science," *American Economic Review*, June 1959, pp. 255–256

10. In "Economics and Business Planning," *Economics and the Policy Maker, Brookings Lectures*, 1958–1959, The Brookings Institution, 1959, pp. 16–17, S. S. Alexander speaks of the economist as a business consultant, "as a specialist in knowing where to begin, and hopefully, where to go from there, in the analysis of a projected situation." "What is required is skill in appraising an analytical problem, in identifying the relevant variables, and in using empirical techniques to estimate them and theoretical analysis to integrate the estimates into a picture of possible future development under assumed conditions. . . .

"An economist may be permitted to believe . . . that a person trained in economics, and more particularly, experienced in economic research, can appraise these problems from a somewhat richer background, relative to the needs of the problem, than can one trained in other disciplines. . . .

"A more fundamental claim that can be made by the economist for special competence in the long-run analysis of business problems is that of familiarity with the general context of these problems and insight into the nature of the relationships."

See also the lecture by C. J. Hitch, "The Uses of Economics," to be published soon by The Brookings Institution.

Current Trends in Political Science

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To attempt an examination of one's chosen field with an eye to its present and future place in a scheme of liberal education is a hazardous undertaking. So much might be said of such an attempt in any field. Where, however, the subject is one that is rapidly changing, is divided by vigorous controversies, and is displaying a variety of apparently disparate tendencies, the hazards attending an effort at assessment are especially great. These are the circumstances in which political scientists are currently working. Under such conditions an interpretation of the field by one who is in some measure a participant in its controversies cannot claim to represent a consensus and is liable to coloration by the preferences of the commentator. It can, however, attempt to avoid partiality and exaggeration, which are at best unhelpful in disputation and at worst seriously misleading to the interested outsider. The following account is offered in that spirit.[1]

The Developmental Background

A field of study rarely, if ever, makes a clean break with its past. Changes, even when they occur rapidly, are seldom total, and current practices as well as contemporary controversies need the perspective at least of acquaintance with their origins if they are to be understood. Since contemporary political science, perhaps in peculiar degree, displays strong continuities with its past as well as sharp changes of recent date, a brief sketch of some of the main lines of its development in the United States will be helpful.[2] No attempt at covering work outside this country is necessary, since until quite recently American political science has been little influenced from abroad. Scholars transplanted from Europe in the past thirty years have been among its most creative workers, but the growing edge of the discipline has been in the United States, and the peculiarities of the field bear a native stamp.

The field of political science in something like its present form is largely a product of the twentieth century, but some sort of instruction

in "politics" has been carried on at the collegiate level for a very long time. Even the little colleges of the colonies had some such offerings. Until well after the Civil War, however, the field was a part of moral philosophy. This emphasis upon political ethics, especially after the Revolution, reflected an understandable concern with the duties of citizenship, not with a systematic analysis of the operations of governmental institutions.

Early in the nineteenth century instruction in politics was somewhat enriched by teaching in the rudiments of law, by study of *The Federalist*, and in some institutions by reading in de Tocqueville. The predominant tone through most of the century, however, was a preceptive and scholastic formalism. This tone is perhaps best represented by Theodore Dwight Woolsey's *Political Science; or, The State Theoretically and Practically Considered*, first published in 1878, seven years after its author had retired from twenty-five years of professing this subject. The book is learned and long, but it is basically a highly abstract treatment of rights and, even in the section on "Practical Politics," a formal, legalistic description of institutions. Despite its formality, which was in the tradition of German *Staatswissenschaft*, the book had a broad sweep and drew freely upon Woolsey's extensive knowledge of the classics and his broad acquaintance with contemporary forms.

The tradition of formal, preceptive speculation was gradually displaced after the Civil War. This change, slow and uneven, was paced by the activities in the new graduate schools at Johns Hopkins and at Columbia, which were breaking away from a preoccupation with legal forms to a concern with "facts." It can be illustrated by Woodrow Wilson's *Congressional Government*, published in 1885, but not by his treatise on *The State*, issued four years later, which was clearly in the traditional mold. The tone of the change was struck, however, less by any American than by James Bryce, whose *American Commonwealth* first appeared in 1888.

This shift toward contemporaneity, toward "fact" and "practicality," reached a kind of peak about the turn of the century and dominated the field until at least the 1930's. In origin it was a reaction, in the name of "realism" and "science," against a legal and philosophical formalism that seemed to disguise the facts of political activity in the United States and to have no bearing upon the conspicuous problems growing out of the new industrialism. In its culminating phase it was essentially a part of the general pursuit of "realism" that was characteristic of the Progressive Movement in literature, in journalism and in others of the social sciences.[8]

Its spirit is suggested by the title of Albert Bushnell Hart's textbook of 1903, *Actual Government as Applied under American Conditions*, which was also the name of his Harvard course.

Significantly, this new realism was not pessimistic or defeatist, but optimistic, confident of progress and actively reformist. It had some fundamental consequences for the discipline, consequences that were mutually reinforcing and are in many respects still evident in the field. The first of these consequences was that, partly as a result of concentrating upon concrete particulars, the inclusive political system tended to be taken for granted. It was treated as given, its basic features and underlying values implicitly assumed. As a result, the ramifying effects of particular reforms, such as the direct primary, could be and were comfortably ignored. Serious, analytical assessment of the direct primary's implications for the system were almost wholly missing from the literature until V. O. Key's work in the 1950's.[4] Particular problems were attacked in isolation and without awareness of their context or of the investigators' implicit assumptions concerning the system as a whole. Yet these assumptions tended to fix the questions asked and to restrict the answers offered, whether the problem involved the reform of electoral machinery, the regulation of private enterprise or the reorganization of local governments. The concerns of the period generally, but particularly of political scientists, were such that they implicitly assumed a kind of atomistic non-system. Preoccupation with describing particulars in isolation from the whole scheme and an obsession with "practicality" led so far away from concern with the political system as a whole that it was virtually assumed out of existence. Some commentators have argued that this tendency to take the system for granted is a special mark of American liberalism in general.[5] True or not, the tendency in the Progressive period left clear marks upon the work of political scientists. Implicitly many of them rejected or avoided the kind of conflict that lies at the base of politics and assumed, or at least seemed to aspire to, a kind of immanent harmony among right-minded men.

A second consequence of the new realism was that, in over-reaction against the old formalism, a concern for theory in any systematic sense—historical, moral or analytical—virtually disappeared. "Theorist" became, as in some quarters it still is, a pejorative term. Practicality and reform were the central matters, and in their particularity they had no need for theory. Albert Bushnell Hart was correctly appraising many of his colleagues as well as the temper of Americans generally when he wrote in

1907 that "The most distinctive American theory of Government is not to theorize."[6] Many scholars concerned with the history of political thought—all the "theory" that was to be found in most institutions—were excluded, not as persons but as scholars, from the main stream of the field's concerns.

The new realism took its "science" seriously, but this third consequence assumed the form, almost inevitably, of a rank, anti-theoretical empiricism. The dominant commitment, even among such pioneers as Frank J. Goodnow, A. Lawrence Lowell and Charles A. Beard, was to the collection of facts and more facts, almost for their own sake. A virtue of this commitment was that it led to a concern for the development and improvement of techniques for the impartial collection and analysis of data. Especially in the decade following World War I, great strides were taken in this direction. Political scientists pioneered in the collection of data by direct interviews with systematic samples of the population. Interesting attempts were made at partially controlled "natural" experiments, at the study of political attitudes through interviews, content analyses and word-association tests, and a great deal was done to explore the problems involved in the quantification of political data.[7] But alongside these technical advances ran the assumption that the facts would speak for themselves—whether in explanation, in reform or in citizenship education, which remained strong as a professional objective. Empirical description, often in association with useful public reforms, became the hallmark of the field.

As a fourth consequence of the new realism, political science became increasingly parochial, engrossed in things American and often necessarily with the minutiae of matters on the domestic scene. A natural implication of the importance attached to practical reforms, this parochialism also reflected the rejection of theory. Among its results, however, was a failure to develop, if not a tendency entirely to neglect, the comparative method as a basic instrument of political enquiry. One of the saving features of the formalists had been their rudimentary interest in comparison, but this concern drew little encouragement from the parochialism of the new realism. "Comparative government" became almost wholly a meticulous description of the constitutions and political structures of particular foreign governments, and what was called comparison in such enquiries amounted to little more than literary gestures. In turn, comparative analysis as a method applicable to the diversities of the American system was undeveloped and unexploited.

The final and somewhat ironic consequence of the new realism was that, given the limitations imposed by particularism, empiricism, parochialism and the rejection of theory, the field did not escape all formalism by rejecting the philosophical formalism of the older tradition. It merely retired into the formalism of concrete description. Anti-theoretical empiricism and a lack of concern for the properties even of the American system condemned American political science to work within, that is, to describe and to reform, the more obvious components of the legal and conventional structure.

These five tendencies, consequences of the new realism that dominated political science at least until the 1930's, necessarily constitute a simplified account of major trends. Though they may be accepted, they do not do justice to the full diversity of activity in the field during this period. Nor do they take account of exceptions to the general tendency. Yet there were exceptions, even among the works of individual scholars, especially the abler ones such as Lowell.[8] Moreover there were dissents or temporarily weak counter-tendencies. Thus Graham Wallas's *Human Nature in Politics*, published in 1908, for all its over-enthusiasm about psychology's potential for political science, was an eloquent rejection of institutional description for its own sake, perhaps more influential on this side of the Atlantic than in England. Arthur F. Bentley, whose *The Process of Government* was published in the same year, was part of the Progressive revolt against the older formalism. But he was equally hostile to many features of the new, so that his first major work is more representative of current trends and controversies than of those that were dominant when it first appeared. Charles E. Merriam's *New Aspects of Politics*, with its characteristic breadth and sensitivity to the implications of work in neighboring fields, appeared in 1925. It was a kind of manifesto for what came to be known as the "Chicago school" of the 1930's. Important though these and other efforts were for future developments, however, they were at the time exceptions to the preoccupations characteristic of political science.

The Sources of Fresh Tendencies

Given the characteristics of the field as it was until at least the middle of the 1930's, it was natural that its subdivisions should diverge. Rather than the parts of an integrated discipline, they were more often a collection of particularities. These sub-fields, fairly uniform over most departments, have been nominally standard for decades. They are four: (1)

American government, containing a number of more or less separate specialties, such as public law, public administration (a newcomer in the 1920's that has shown at times strong secessionist inclinations), parties and voting behavior, and state and local government; (2) Foreign or "comparative" government; (3) International relations, which has displayed tendencies toward either separatism or inter-disciplinary organization; and (4) Political theory or the history of political thought.

Some purpose might be served by an attempt to describe developments within each of these. Such an undertaking, however, would strain the limits of space and might become a bibliographical essay of little value to the interested outsider. More important, such seriatim treatment would obscure important signs of integration within the discipline. At present they are not much more than ambiguous signs, existing perhaps chiefly in the eye of this observer and certainly liable to exaggeration, but they are of more consequence, as potentialities if not as portents, than the details of particular specialties. The distinguishing characteristics of a field lie in the questions that it asks, and any tendency toward disciplinary integration should be evident in the number and types of questions that are common to its sub-fields.

The sources of change and integration within political science in recent years are chiefly a series of events and experiences that subtly but rather directly challenged the adequacy of older notions and practices by destroying an implicit optimism that had pervaded the field and by compelling attention to fundamental but neglected problems, such as the conditions of political change. The most obvious of these was the direct governmental experience acquired by an unprecedented number of political scientists during the New Deal and World War II. These transplanted academics not only acquired first-hand acquaintance with Washington problems but also frequently found themselves in mutually instructive collaboration with men in other disciplines. The assignments of political scientists varied over a wide range and, given the demand for educated manpower, were not confined to specialists in public administration, although the impact of contact with bureaucratic reality may have been particularly important for this sub-field.

Less obvious but probably of greater consequence were the sweep of events on the world stage from the 1930's through World War II and the scope and rate of change in the years since 1945. The rather naive optimism and implicit faith in progress that permitted political science in the first decades of the century to become preoccupied with concrete par-

ticularities at the expense of fundamental problems could not pass unchanged through these developments. Crisis and the rapid alteration of familiar forms normally stimulate fresh intellectual activity by denying to tacit assumptions the authority of absolutes. Something of this sort underlay the emergence of "realism" in political science following the Civil War. On a small scale the disillusion that followed World War I, evident, for example, in the work of Walter Lippmann, had a similar origin, though the effects among political scientists in this instance were moderate.[9] Apparently they were not severe enough, at any rate, to be incompatible with a transfer to the League of Nations and other international organizations of both the optimism of the earlier period and the uncritical assumptions that went with it.

Charles Merriam's enthusiasm in the 1920's for a more scientific political science may have been in part a search for a stable means of understanding in an unstable world. Certainly the years of Hitler's sway compelled most political scientists to re-examine what previously was unconsciously assumed, to search not so much for stability in the world as for reliable intellectual control of instability. If the governmental changes in the United States during the New Deal could with difficulty be accommodated to earlier modes of thinking, the parallel events in Europe, not only in Germany and Spain but in France and even in Great Britain, liquidated the intellectual utility of the rootless, untheoretic concern for descriptive detail that hobbled the student of "comparative government."

In the period since World War II the obsolescence rate of older notions and practices has further increased, although the replacement rate may not have been so rapid. The source of change has been not merely the drastically altered structure of world politics since Potsdam, though this has been fundamental to the change. More specifically, the involvement of the American government and many political scientists in the problems of exotic areas over the globe has, for example, strained the "rules" of public administration, revealing their culture-bound bases. The necessity for coming to grips with the problems of political development in these areas has invited attempts to recast the comparative method. Finally, to mention only one additional example, developments in physical science and technology have compelled attention to their political implications, domestic and international, especially but not exclusively in relation to the new political position of the military establishment.

Some critics may argue that these changes merely illustrate the chronic susceptibility of American social science in general and political science

in particular to "fashions in topics and methods."[10] Admittedly they have involved some elements of faddism, especially when foundation executives and government contract officers have displayed more zeal than prudence. But what appears as topical faddism may also be interpreted as a responsible focusing of research upon problems relevant to urgent matters of public policy. They may also supply a reasonable basis for confidence that these new searchings and re-examinations may emerge with net gains in the growth and competence of the discipline. In any case, the searching and re-examination are occurring, despite some excesses, some floundering in jargon and neologism, and some opportunism.

Revived Concern with the Political System

Four broad and closely related tendencies resulting from these stimulants to change can be detected in contemporary political science. The first and perhaps the most significant of these is a revived concern with political systems as such.

It is probably correct to say that political scientists have long assumed that "the phenomena of politics tend to cohere and to be mutually related" and thus constitute a system.[11] For reasons that have already been indicated, this assumption was usually unexpressed and, more importantly, it was largely unexplored. The events of the past three decades have forced expression and explicit examination of that assumption. They have compelled attention to the properties of political systems and subsystems, including the conditions of their alteration. These responses in turn have given promise of creating something like a common disciplinary enterprise out of a collection of divergent sub-fields.

This revived concern with system was not wholly unprecedented. Thus it is accurate to say that the Chicago school of the 1930's represented not only the negative characteristic of rejecting a narrow concern with institutional description but also the positive merit of an interest in the coherence of political phenomena. For example, the first edition of Frederick L. Schuman's *International Politics*, which appeared in 1933, broke with the moralism and mechanical description of the previous decade in its conception of international politics as a discernible system in which the struggle for power occurred without the benefit of integrating institutions. Similarly the many-faceted work of Harold D. Lasswell, then and since, focuses distinctively upon types of systems, their properties and conditions of change.[12] Also in the 1920's and 1930's had appeared several naturalistic studies of policy-making processes in the

United States, whose perspectives were wide and whose approach was analytical.[13] These did not represent the main stream of interest, however, until after World War II.

A distinctive feature of this new emphasis upon systems is a concern with a wider range of variables—psychological, economic, demographic —than is automatically provided by conventional institutional, especially legal, categories. Associated with it is a tendency toward asking the same or closely related questions in most of the traditional sub-fields. A considerable proportion of the contemporary literature can be analyzed, with different degrees of emphasis and systematic concern, in terms of attempted answers to such questions as the following: Who (or what) are the relevant actors in the system (or situation)? What are their goals, perceptions and motivations? What, broadly, are their resources? What strategies, including methods of choice and the norms of institutionalized channels, are available to these actors? Under what kinds of conditions are any of these subject to change? Conceptions or definitions of "political system" vary from one work to another, but not only are these differences usually reconcilable, they are in a sense often complementary, reflecting variations in the degree to which the question of change, for example, is central to a particular author's concerns.[14]

In consequence, problems and concepts increasingly are becoming interchangeable, and are being so recognized, whether the immediate, empirical concern is the emergence and conditions of survival of supranational blocs in world politics, the making of policy at the national level, or the power structure of the local community.[15]

The questions that currently command the attention of many political scientists are not confined, of course, to those dealing directly with political systems. Nevertheless, when some institutional segment or process is the focus, the trend increasingly is to examine such restricted phenomena in the context of a relatively explicit conception of the inclusive system. Partly because of this trend, moreover, such analyses are less often treated as isolated, independent problems. Rather they are likely to be concerned with interdependences between the restricted segment and the remainder of the political system. A good example is the study of voting behavior, which easily and hence usually has been treated in isolation, as an end in itself, with the enquirer rarely going beyond an attempt to explain why types of electors behave as they do. Or it has been merely an aid to explaining in historical terms why particular elections resulted as they did. Or electoral analysis has served as a launching point for reformist moral-

izing over participation rates, levels of electoral ignorance, or the alleged dangers of political deception and ambiguity. In contrast, a series of recent studies has looked upon the analysis of voting as a means of examining the reciprocal effects of types of individual voting behavior, the political system as a whole, and various intervening or constituent structures such as party organizations and organized interest groups.[16] The same sort of tendency is evident in some studies of decision-making processes; it is apparent also in analyses of particular roles and institutions and even to some extent in studies in such stylized areas as public law.[17]

This convergence of concern with the political system is one of the chief influences favoring a reduction of the artificial barriers between specialized subdivisions within the field, without eliminating a fruitful and necessary division of labor. Thus within an area such as international relations, such sub-specialties as politics, law and organization are now rarely viewed as autonomous sub-fields but rather as related and basically inseparable means of approaching a variety of problems, whether of current policy or of analytical interpretation.[18] Similarly, studies of the making of foreign policy have become a focus of common concern and complementary analysis by students of domestic politics, for whom this area usually has been at most a minor sideline, and those specializing primarily in international relations.[19] Public administration has shown similar tendencies. A field that has suffered from a parochial emphasis upon the American scene, from an over-commitment to "practicality," traceable to its roots in municipal reform and in the early scientific management movement, and from premature formalizing, public administration tended to develop in isolation. With the re-emergence of the political system as a common frame of reference, this sub-field has shown increasing signs of a return to the main stream of political science. Since 1945 this tendency has included the introduction of international and inter-cultural dimensions in administration and a consequent convergence of interest in the study of international politics and in non-American systems.[20]

Similar tendencies are apparent in other areas, including political theory, which will be discussed in more detail in a later section. These are not fully evident in teaching, especially at the undergraduate level, because they have not yet altered the conventional pattern of most textbooks. But the political scientist as a scholar no longer finds it easy or profitable to confine his attention to the literature that happens to bear the label of his special sub-field. The renewal of interest in the political system means, among other things, that he is likely to find relevant and

stimulating ideas, whether for research or for teaching, almost anywhere within the broad field.

Re-emergence of the Comparative Method

Parallel to and partly in consequence of the new consciousness of the political system, a number of related developments have occurred. One of the most important of these is a fresh concern with the comparative method. The aim of this quite conscious movement is increased conceptual sophistication, an objective that is eminently valuable whether or not it is achieved in a measure sufficient to produce adequately tested generalizations. The parochial and particularistic trends in political science in its youthful years were, as we have noted, inhospitable to any genuine use of the comparative method. With one or two conspicuous exceptions, as in the work of Carl J. Friedrich,[21] what passed for "comparative government" was no more than description, in series, of the formal governmental structures of several European countries. Consequently most attempts at comparative analysis worked with legally established institutions that bore a nominal resemblance but at a less superficial level were often dissimilar in function. These attempts were limited and frequently sterile because they lacked the means, in data and especially in concept, of dealing with differences in function that belied the common labels.

Since World War II considerable dissatisfaction with this state of affairs has been expressed.[22] Such criticism reflected the well justified feeling that even the limited validity of customary attempts at comparison was subject to repeal by the next day's press reports. It further reflected the less frequently expressed view that such conventional efforts often exaggerated superficial differences while ignoring more fundamental similarities. For example, they tended to neglect responses to the problems of industrialization that are significantly comparable, partially transcending and partially conditioned by differences in ideology, in inherited institutional forms and in national resources. This view is not yet a common one, but it has appeared in some significant recent studies. A good example is the collaborative work by Robert A. Dahl and Charles E. Lindblom, *Politics, Economics, and Welfare,* published in 1953. Not a study in comparative government in the usual sense, this is a systematic analysis of the basic social processes underlying politico-economic systems as different as those of the United States and the Soviet Union.

The new concern with comparative method has been reflected, appropriately enough, in a number of studies aimed at identifying the func-

tions and the conditions of variation in the governmental systems and political processes within the United States at the state and local level. Through such investigations the opportunities on the domestic scene to improve methods and to increase conceptual sophistication are beginning to be exploited.[25] Studies at the state and local level have their special difficulties, but they afford the considerable advantages, in contrast with trans-national comparisons, of providing a large number of cases while reducing the range of cultural variables that must be controlled. Combined with relatively easy access to sources of data, these assets promise gains for the field as a whole as well as for the study of American politics.

But the strongest stimulus to the development of comparative method has been the growing interest in non-Western areas, especially the new and "developing" nations. Adequate study of these nascent systems will not permit either taking the characteristics of the system for granted or applying the categories of analysis conventionally employed in discussions of the West. Not only are the new states different in fundamental respects, but also most of them are undergoing dramatic change. The assumptions implicit in taking for granted the nature of the inclusive political system are likely to be treacherously misleading when rapid and largely unplanned change occurs. Such assumptions have handicapped the analysis of Western politics; they are less than useless in dealing with the fluid patterns in the new nations. These require categories and concepts less restricted by temporal and cultural limitations. As they are developed, however, the relevance of such analytical schemes will not be restricted to the new systems.

Thus the fundamental task of this kind of analysis is to develop a frame of reference or conceptual scheme that will permit the comparison of whole systems in terms of variations in attributes that compose the scheme and are common in some degree to all political systems, Western and non-Western, developed and developing. The difficulties admittedly are tremendous, but some signs of advance are appearing. Some of the most promising of these rely heavily upon the functional theories of sociology and cultural anthropology. Thus the recently published work of Gabriel Almond and his collaborators attempts to employ a scheme composed of a limited number of generic functions. Comparisons deal with frequencies in the performance of the functions, the kinds of structures performing them, and the "styles" or modes of performance.[26]

This kind of functional analysis is an extension of tendencies previously evident in political science.[27] How far it may be able to go, how-

ever, is debatable. The pitfalls and obstacles are many. In the first place, the abstract attributes composing the scheme may impose a task of subcategorization and identification of indicators so complex as to be unmanageable. Secondly, the enterprise may involve so large an investment in tooling-up and a consequent postponement of empirical testing so prolonged that it may bog down in a morass of scholastic verbalism. Thirdly, the apparent simplicity of the scheme may invite premature, oversimplified and undiscriminating attempts at application whose results may be as superficial as those from older methods of comparison.

However far such efforts may be able to go, they are unmistakably valuable on several counts. First, they help to make explicit the kinds of assumptions underlying much that is currently written of political systems or their segments, whether offered in a comparative framework or not. Secondly, they set the problem of useful comparative analysis in clear, if somewhat forbidding, terms. The attempts themselves may provide no firm solutions, but by stating the problem they establish a discriminating criterion of performance. Thirdly, they re-emphasize the central place of theory in any field that has reached the point of recognizing reliable classification as the first step toward understanding. Finally, they may help to avoid some of the excesses threatened by the current vogue of "area studies," by reminding the incautious that crude "facts" are no more a substitute for disciplined analysis when they derive from exotic regions than when they are found in the immediate neighborhood. Manhole counting does not become science by being located in Leopoldville.

Re-Commitment to the Goal of Science

The revived interest in the analysis of political systems and the new concern for comparative method are associated with a third trend, a re-commitment to the goal of scientific method in the study of politics. This trend is awkward to discuss, not only because it seems to require a precise indication of what is meant by the term "science," which is not attempted here, but also because the commitment is far from uniform throughout the field and hence is the focus of a running controversy in which the issues are rarely defined and, if joined, are still more rarely resolved. The commitment is evident, however, and cannot be omitted from any reasonable account of tendencies in the field. Even a cursory examination of the leading journals over the past dozen or so years will reveal the trend and will show, as in other social science fields, an increasing sophistication about what the commitment requires.

Critics within the discipline often regard the tendency, with mixed anxiety and hostility, as a contemporary illustration of a delusive aspiration that goes far back in American thought, especially political thought.[26] The current trend can be viewed more generously and more accurately as one kind of conscientious effort to find order and understanding in a complex and changing world. The mode of that effort, moreover, appears to be quite reasonable, whether the motivating aspirations are rewarded or not. If, with C. P. Snow, one accepts the seeping of scientific thought into all fields as the characteristic fact of contemporary intellectual life, a commitment to the goal of science may be the most reasonable path to order and understanding in the political realm.

Controversy over the goal of a scientific study of politics has focused on the label, "political behavior." Like the symbols in most holy wars, the term has no very consistent meaning. Under this banner, with varying degrees of reluctance, move most of those participating actively in the trends already discussed, most of those interested in the development of empirically testable theory, all who espouse the extensive use of quantitative methods, and most of those who are persuaded that political science has much to gain from the techniques and concepts used in other "behavioral" sciences. As this listing suggests, however, the political scientists thus classified do not necessarily share the views of all others in the camp. For some, "political behavior" is a sub-field, usually distinguishable by techniques involving quantification but sometimes confined to the application of such techniques to the study of voting behavior. For others, it is rather an approach to political phenomena, systematic but not necessarily quantitative. Among the latter, some see the approach as concerned broadly with activity in the political realm, while others regard it as concerned only with the psychological aspects of such phenomena.

Nevertheless, the common tendencies in the new commitment are fairly clear. The first is to concentrate upon the identification of uniformities of activity and their conditioning circumstances in the political realm. A second is to take as units of analysis the observable or inferrable interactions of individuals and groups in the political sphere, including their hypothetical determinants. And a third is to deal with these units by means of carefully defined concepts and empirically testable propositions.

The range of applications attempted through this approach is wide.[27] It, of course, includes research on the motivations and choices of the electorate, but it extends to studies of the roles of major political actors, such as legislators, and their controlling conditions and implications; studies of political socialization and the processes of recruiting individual

political actors; the functions and properties of extra-governmental groups; and the behavior and internal dynamics of administrative and other governmental organizations, to mention only the most common instances. In addition, the influence of the "movement" is evident in a still wider variety of studies, reaching into most of the standard areas in the field.

Among the zealots on the side of "political behavior," as so often in academic controversies over new tendencies, one can detect a disposition to establish rigid and often fruitless criteria of the new orthodoxy. A worthy emphasis upon empirically testable propositions may thus be associated with a less estimable denial of relevance to any other kind of statement. This is a counterpart of the view that all quantification is good and that all data collected by a favored technique, such as the sample survey, are valuable, regardless of the uses to which they are put. Similarly an appropriate pursuit of analytical categories not supplied by the official structure of the political system may develop into a constricting tendency to regard all descriptive analyses of such structures as "institutional" and therefore useless. This extreme thus places all such enterprises into a category falsely opposed to "behavioral" analyses, when in fact the two are in many respects complementary.[28]

Such excesses as these both reflect and stimulate equally extreme positions among the non- or anti-behaviorists. Some of the latter would deny, often quite explicitly, that the field is or should be concerned with the identification of uniformities, with patterned behavior and its conditions, and would assert that its proper preoccupation is with the unique, the specific and the concrete. At times the old distinction between "nomothetic" and "ideographic" sciences has been resurrected and these categories treated as absolutes rather than as indicators of differences in degree and tendency. At other times, as in the work of Eric Voegelin, one encounters an insistent rejection of empiricism and "science" in favor of a religio-philosophical preoccupation.[29] More subtle critics are sometimes disposed so to emphasize complexity and diversity in the world of politics as to make an ideal of skepticism and to exalt practical wisdom over rationalism in any form.[30]

At least two unfortunate consequences of these controversies can be identified. Because the allegedly opposing positions are often put in extreme terms and the apparent issues are in consequence frequently false, those opposed to the "behaviorists" can easily persuade themselves that they need not reckon with anything that can be given this label, and

sympathizers with the new orientation may find it easy not to face some of the dangers in their commitment. The second is probably more serious, because ignoring these dangers could stunt the growth of an otherwise fruitful enterprise. Several such dangers can be noted. First is that of espousing rigor at the expense of relevance, that is, of pursuing refined or fashionable techniques for their own sake, however trivial the substance of the enquiry. Second is the related danger of allowing a favored technique to set the limits of research interest, so that problems to which it is not applicable are discarded or ignored. The research motivation then becomes, not what problems demand solution by means that may be devised, but what questions can this instrument be put to, regardless of their objective priority. Perhaps a third and lesser danger is that of forgetting the difference between a language that is genuinely technical and a vernacular that is merely obscure.

These dangers are greatest, perhaps, in cases of undiscriminating borrowing of concepts and techniques from other social sciences, especially if the notion borrowed is more clever than precise, where it is misapplied by the borrower, or where he ignores the analogical character of the reasoning implicit in the transaction. Despite occasional missteps, however, most borrowing from related disciplines has been productive rather than diversionary or sterilizing. Examples are difficult to specify, since even in a single work materials from several other fields may be used. It is perhaps accurate to say, however, that the most persistent influences have come from the various branches of psychology, going back at least as far as Lasswell's early work.[81] Recent examples would include Almond's research on the social and psychological sources of adherence to the Communist Party and some work in the area of administration.[82] The use of psychological materials is also evident in the broadened curiosity that has increasingly characterized writing on international relations.[83] But much the most significant influences from psychology, upon both concepts and techniques, have appeared in the studies of electoral behavior, thus far the most fruitful juncture of political science, psychology and sociology.[84]

Almost equally strong influences have come from sociology and cultural anthropology in recent years, although the patterns of exchange are not easily specified, especially where an inter-disciplinary appearance disguises, as in the case of some so-called political sociologists, the parochial fact that a political scientist is operating under another departmental label. But genuine borrowing would include not only the use of a number of European sociologists such as Durkheim, Simmel, and Max Weber, but

also adaptations from contemporary "functionalists" such as Talcott Parsons and Robert Merton. Instances of the latter are particularly conspicuous among political scientists working on underdeveloped countries and among those more broadly concerned with improving comparative analysis.[*]

Borrowing from economics has been somewhat less active, perhaps because segments of economic theory are less adaptable to piecemeal incorporation into other fields, but influences from economics can be seen nevertheless in studies of decision-making and control processes and, along with material from other areas, in work on organizational analysis.[*]

The major implication of these inter-disciplinary tendencies for present purposes is that the re-commitment to the goal of a scientific study of politics is associated with comparable, and in some cases stronger, trends in related fields. How far this commitment may be able to carry in political science is uncertain, to say the least. It is likely, however, to remain strong as long as associated tendencies, in and outside the field, persist in the general directions that have been sketched in these pages.

Renewed Emphasis upon Political Theory

As the "realism" and crude empiricism that dominated political science in the first three or four decades of this century were inhospitable to theory in any form, those still disposed toward theory, primarily students of the history of political thought, were thrust out of the main stream of the discipline. In most departments their interests were relegated to a marginal area where it was permissible to engage in elegant refinements of doubtful utility. They were, in a sense, assigned the suspect world of values—the "ought" in contrast with the useful world of the "is"—a pattern parallel to but less severe than the exile of jurisprudence by schools of law. Partly in consequence, "theorists" tied themselves more closely to related and more congenial fields, such as philosophy, the classics and history. They thus tended to confirm their isolation. More seriously, a tendency toward worshipful antiquarianism among many specialists in political thought easily reinforced students in the attitude that "theory" was something one ought to know about but not something relevant to the serious pursuit of an interest in political science. Or the "theorists" adopted a historicism that in effect held that political ideas are so completely the product of conditions at the time of their appearance that they are significant largely for the part they have played in their appropriate historical periods. Such a view easily persuaded students that the classics

of political thought had no bearing upon the present except as elegant ornaments.

The three trends already discussed—a focus on the political system, an interest in the comparative method, and a commitment to the goal of a science of politics—necessarily have involved a new and rather different concern for matters theoretical, if not broadly philosophical. The changes that produced these trends unavoidably gave to theoretical enquiry a new urgency that justifies classifying it as a fourth major trend. As Hans Morgenthau has observed, "Science is theoretical, or it is nothing."[87]

In almost every sub-field theoretical questions are receiving increasing attention. The generality of this emphasis, along with sources of convergence in the field that have already been noted, has helped significantly to reduce artificial barriers between conventional sub-fields. One can safely say that today the leading textbooks in each of these areas—international relations, comparative government, public administration, American government and the others—show a degree of care at least about consistency of assumption and coherence of expository framework that was rare twenty-five years ago. Although this indicates nothing about the theoretical adequacy of these efforts, the change is significant. Such a shift in the character of introductory materials, moreover, reflects an increased amount of more technical writing concerned explicitly with theoretical problems in the sub-fields and in political science as a whole.

In none of these areas, of course, does one find consensus on a single theoretical position. Differences are numerous and sometimes sharp. They are likely to continue. Nevertheless, one can reasonably expect that, as theoretical positions are stated with clarity, it will be possible more frequently to identify crucial points of difference and to specify the requirements of choice and the limits of reconciliation.

As might be expected, differences are greatest, first, among those whose theoretical concerns are most inclusive and, second, between these and others whose theories are more segmental. Differences within the specialized, less inclusive substantive areas are less striking and almost certainly less numerous. Within such restricted fields as electoral behavior, for example, an approximation of consensus may be said to exist. Such a convergence of view unquestionably has multiple origins. In part it may reflect a relative simplicity in the central problems of such areas. Additionally, and perhaps primarily, it is a consequence of the opportunity in a sub-field such as voting to make a very large number of observations over a relatively short time span and to test one's speculations by observa-

tions that are relatively standardized if not perfectly precise. More inclusive concerns, for example at the level of whole political systems, must necessarily work with a relatively small number of cases and with comparatively unstandardized and imprecise observations. Hence the incidence of untested speculation is likely to be high in such areas, and the variety of theoretical positions is certain to be considerable.

In these circumstances tensions and apparent incompatibilities between theoretical positions with varying degrees of inclusiveness are inevitable. Inescapable also, at least in the short run, are such differences among theories of relatively broad scale. These, it should be noted, have their parallels in more integrated disciplines, even in the natural sciences. Witness the divergence of scientific views concerning the origin of the universe. If, however, the bases of these differences are recognized, and especially if, after the fashion of the physical sciences, the requirements for their resolution are understood and accepted, theoretical incompatibilities can have constructive consequences for the discipline even though they may not be easily reconciled. An increased general concern with theory should carry at least this promise.

The renewed emphasis upon theory is not confined to matters growing directly out of empirical investigation. It includes a tendency for the classics of Western political thought to be recalled from exile along with other theoretical concerns. The routes of this return are several. First has been a trend, common to much of social science, away from crude ethical relativism. It may not be the case that extreme relativism is a consequence of "realism" and its untheoretical inclinations. Nevertheless, the evidence suggests that as empirical, analytical theory becomes more sophisticated and attempts to become more inclusive, it raises inescapable questions of value.[28] Alternative theoretical and practical positions are seen to have consequences in the realm of value, a realm that has been explored repeatedly in the history of political thought. Although the classics in the tradition have a relevance more extensive than this, their re-introduction as this sort of question regains prominence is appropriate. It is also peculiarly appropriate, perhaps, that a conspicuous part in this revival has been played by a number of scholars who made their academic careers in the United States after receiving all or most of their training in Europe and especially in Germany, where, among other favorable circumstances, the rank empiricism that has characterized American political science was largely absent.[29]

Secondly, as theoretical concerns of various sorts become more nearly

central in the discipline, more political scientists are likely to find suggestive insights and useful observations in the classics of the field, as in any other sort of historical material. More important, to the extent that his theoretical problems are at base generic problems of political analysis, such as the conditions of obedience, the requirements of consensus, and the sources of legitimacy, the contemporary political scientist can find in the classics of the field means of sharpening his formulation of problems and of strengthening his critical powers for dealing with them. The alternative is likely to be a kind of wasteful arrogance. One sign of an increased theoretical sophistication in political science may be that it is becoming more at home with its classics.[40]

Thirdly, as theoretical concern becomes more formal, to the point where much of it concentrates upon constructing, manipulating and examining the consequences and utilities of various "models" of political systems and processes, such theorizing will find in the classic literature not only suggestive leads but also what amount to ready-made instances of direct relevance to their concerns. Little of this sort has appeared in recent writing, and it may not emerge in volume very rapidly, but the appropriate pre-conditions are present, and examples may be expected to recur.[41]

These several signs of a revival of theory do not mean that the field will abandon a division of labor reflecting differences of preference, of training and of aptitude. They do promise, however, an end to the crippling effects of an attempt to deal systematically with the phenomena of politics without a concern for systematic theory. What is emerging is a healthy relation between theory and empirical investigation, a relation of constructive interchange. A great deal of what political scientists do will continue to be concrete and empirical, and much of it will not be aimed explicitly at theoretical questions. But if the tendency toward a renewed emphasis upon theory retains its strength, these workaday efforts will be more valuable for being conducted in an atmosphere of concern for questions of greater generality.

Qualifications and Projections

This statement of converging trends in political science admittedly simplifies the diversities in the field. Although hopefully the lines of development identified in these pages do not distort reality, they do not fully represent it. The central tendency toward integration around system, comparison, science and theory is at best one feature of a distribution

whose standard deviation is large. Thus tension between the goal of a political science of broad analytical competence and the goal of immediate practicality remains. If the trends have been assessed correctly, it should be less acute than in the past, but it may not entirely disappear. It emerges, for example, in the guise of a demand for "training in citizenship" and for courses in "practical politics." Neither of these, to be sure, is inherently undesirable and both may have considerable educational value if they are treated as means of stirring an interest in problems that are basic to political culture rather than as ends in themselves, as cheap substitutes for disciplined understanding. Much the same can be said of some undiscriminating uses to which the rich and growing body of contemporary case materials may be put.[42] Properly used, these can be a powerful stimulus to critical analysis, but in unqualified hands they may produce intellectual anarchy in the guise of practicality and realism.

The trends suggested here seem to be gaining strength. As they do so, they will disturb established habits of research and teaching, and on that account they are not likely to be viewed with unanimous enthusiasm. It may be well, therefore, to suggest that they do not amount to a betrayal of the ancient verities. These trends are not hostile, for example, to the goal of civic education in any sense of that term that is genuinely consistent with the purposes of liberal education. Rather, to the extent that they contribute to a more appropriate distinction between the persistent and the ephemeral, between the general and the particular, these trends are likely to result in a more sophisticated understanding of government as the central problem of human culture. Nor are these tendencies inconsistent with the voluntaristic traditions of the field. They may not present political man as the complete master of his fate, but they are wholly compatible with the view that within incompletely defined limits man can make responsible choices. Finally, these trends do not abandon such virtues as may attach to the less ancient tradition of "realism." They are not an attempt to escape from reality. Rather they represent a series of mutually reinforcing efforts to come more effectively to grips with the increasingly complex reality of the contemporary world.

To the extent that they develop from promising tendencies into dominant characteristics of the field, the current trends in political science may have a variety of consequences. Straight-line projections from them would be imprudent, but one may with some confidence anticipate that they will encourage in the next decade or so a number of attempts at genuine synthesis. These may not successfully include the

whole of the traditional field. Almost certainly they will be built upon a variety of frameworks or theories. Hopefully these may be compatible, but whether they are or not, they will represent efforts at ordering the materials of the field in systematic fashion. Clearly this is the direction in which the trends point. The field may not be fully ready for bold synthesizers, but the materials are at hand, and candidates are not likely to be long in coming forward.

NOTES

1. Several surveys of the field, its problems and tendencies, have appeared in the past fifteen years. See *Contemporary Political Science* (1950), *Goals for Political Science* (1951), David Easton, *The Political System* (1953), Dwight Waldo, *Political Science in the United States of America* (1956), Charles S. Hyneman, *The Study of Politics* (1959), and an *ex parte* essay by Bernard Crick, *The American Science of Politics* (1959).

2. These paragraphs draw heavily upon Anna Haddow, *Political Science in American Colleges and Universities, 1636–1900* (1939), and incidentally upon the early chapters of Crick, *op. cit.*

3. See Richard Hofstadter, *The Age of Reform* (1955), pp. 198–199, and Morton G. White, *Social Thought in America* (1949).

4. V. O. Key, Jr., *American State Politics* (1956), chaps. 4–6.

5. Louis Hartz, *The Liberal Tradition in America* (1954) and Crick, *op. cit.*, pp. 157 and *passim.*

6. Albert B. Hart, "The Growth of American Theories of Popular Government," 1 *American Political Science Review* 4 (August, 1907), 560. Crick, *op. cit.*, pp. 96–97, unjustly attributes to Hart full approval of this disposition.

7. Since this paper is not intended to be a bibliographical essay, citations will be confined to illustrations only and should not be regarded as in any way exhaustive. For examples of the developments cited, see Charles E. Merriam and Harold F. Gosnell, *Non-Voting: Causes and Methods of Control* (1924), Harold F. Gosnell, *Getting Out the Vote: An Experiment in the Stimulation of Voting* (1927), Leonard D. White, *The Prestige Value of Public Employment in Chicago* (1929), and Stuart A. Rice, *Quantitative Methods in Politics* (1928).

8. For example, his *Public Opinion and Popular Government* (1913) and his *Public Opinion in War and Peace* (1923).

9. Lippmann's *Public Opinion* (1922) and even *The Phantom Public* (1925) did not entirely abandon his earlier, more optimistic premises.

10. The phrase is from Crick, *op. cit.*, p. 112.

11. David Easton, *op. cit.*, p. 97.

12. Especially, perhaps, his *Politics* (1936) and, with Abraham Kaplan, his *Power and Society* (1950).

13. For example, Pendleton Herring, *Group Representation Before Congress* (1929) and *The Politics of Democracy* (1940); E. E. Schattschneider, *Politics, Pres-*

sures and the Tariff (1935); and V. O. Key, Jr., *Politics, Parties and Pressure Groups* (1st edition, 1942).

14. Compare the conceptions in Morton D. Kaplan, *System and Process in International Politics* (1957), Gabriel Almond and James S. Coleman, eds., *The Politics of the Developing Areas* (1960), David Easton, *op. cit.*, and Karl W. Deutsch, *Nationalism and Social Communication* (1953).

15. On this score compare Ernst Haas, *The Uniting of Europe* (1958), Gabriel Almond, *The American People and Foreign Policy* (1950), Robert A. Dahl's forthcoming *Power and Democracy*, and Wallace S. Sayre and Herbert Kaufman, *Governing New York City* (1960).

16. Outstanding examples, using conventional methods of aggregate analysis, are V. O. Key, Jr., *Southern Politics* (1949), and *American State Politics* (1956). The best example of studies relying primarily upon interviews with samples of individuals is Angus Campbell, *et al.*, *The American Voter* (1960).

17. See especially Robert A. Dahl and Charles E. Lindblom, *Politics, Economics, and Welfare* (1953); also Richard C. Snyder, *et al.*, *Decision-Making as an Approach to International Relations* (1954). On the treatment of particular roles and institutions in the context of the system, see Richard E. Neustadt, *Presidential Power* (1960) and Victor G. Rosenblum, *Law as a Political Instrument* (1955).

18. On this point see the illuminating essay by William T. R. Fox and Annette B. Fox, "The Teaching of International Relations in the United States," a paper prepared for the Conference of the Atlantic Treaty Association, Paris, 1960. For illustrations see Percy Corbett, *Law in Diplomacy* (1959), Inis Claude, *Swords into Plowshares* (1956), and Morton A. Kaplan and Nicholas de B. Katzenbach, "The Patterns of International Politics and of International Law," 53 *American Political Science Review* 3 (September, 1959), 693–712.

19. Bernard C. Cohen, *The Political Process and Foreign Policy* (1957), Gabriel Almond, *The American People and Foreign Policy* (1950), and Roger Hilsman, "Congressional–Executive Relations and the Foreign Policy Consensus," 52 *American Political Science Review* 3 (September, 1958), 725–744.

20. Dwight Waldo, *The Study of Public Administration* (1955), and Harold Stein, ed., *Public Administration and Policy Development* (1952).

21. See his *Constitutional Government and Politics* (1937).

22. See Roy C. Macridis, *The Study of Comparative Government* (1955) and the literature cited there.

23. In addition to the work of V. O. Key, cited in note 16, see Duane Lockard, *New England State Politics* (1959), Leon Epstein, *Politics in Wisconsin* (1958), Joseph A. Schlesinger, *How They Became Governor* (1957), and the forthcoming collaborative study of state legislators by William Buchanan, Heinz Eulau, Leroy Ferguson and John C. Wahlke.

24. Gabriel Almond and James S. Coleman, eds., *The Politics of the Developing Areas* (1960). See also Rupert Emerson, *From Empire to Nation* (1960), and Daniel Lerner, *The Passing of Traditional Society* (1958).

25. Almond cites as examples of such tendencies Robert A. Dahl, *A Preface to Democratic Theory* (1956), V. O. Key, Jr., *Politics, Parties, and Pressure Groups* (4th ed., 1958), and David B. Truman, *The Governmental Process* (1951).

26. As, for example, is suggested by the title and stated in the contents of Bernard Crick, *op. cit.*

27. For a limited number of examples, see Heinz Eulau, *et al.*, eds., *Political Behavior* (1956).

28. Such complementarity is especially evident when an analysis employs a wide range of contextual variables. See, for example, Merle Fainsod, *How Russia Is Ruled* (1953) and Robert Scalapino, *Democracy and the Party Movement in Pre-War Japan* (1953).

29. See Voegelin's *The New Science of Politics* (1952).

30. The best example being Michael Oakeshott of the London School of Economics and Political Science. See his "Political Education," in Peter Laslett, ed., *Philosophy, Politics, and Society* (1956), and Neal N. Wood, "A Guide to the Classics: The Skepticism of Professor Oakeshott," 21 *Journal of Politics* 4 (November, 1959), 647–662.

31. Such as his *Psychopathology and Politics* (1930) and his *World Politics and Personal Insecurity* (1935).

32. Gabriel Almond, *The Appeals of Communism* (1954) and Herbert A. Simon, *Administrative Behavior* (1947).

33. For example, Frederick S. Dunn, *War and the Minds of Men* (1950).

34. See Angus Campbell, *et al.*, *op. cit.*, Eugene Burdick and Arthur J. Brodbeck, eds., *American Voting Behavior* (1959), and V. O. Key, Jr., *Politics, Parties and Pressure Groups*, chaps. 19–21.

35. For example, David Apter, *The Gold Coast in Transition* (1955), Gabriel Almond and James S. Coleman, eds., *The Politics of the Developing Areas* (1960), and Samuel H. Beer and Adam B. Ulam, eds., *Patterns of Government* (1958).

36. See Robert A. Dahl and Charles E. Lindblom, *op. cit.*, a major instance of fruitful collaboration, and James G. March and Herbert A. Simon, *Organizations* (1958). Anthony Downs, in *An Economic Theory of Democracy* (1956), uses an economic model in connection with a limited range of political problems.

37. Hans J. Morgenthau, "The Nature and Limits of a Theory of International Relations," in William T. R. Fox, ed., *Theoretical Aspects of International Relations* (1959), p. 16.

38. On this point see David Easton's discussion of the evolution in Lasswell's views, "Harold Lasswell: Policy Scientist for a Democratic Society," 12 *Journal of Politics* 3 (August, 1950), 450–477.

39. For example, Franz Neumann, *The Democratic and the Authoritarian State* (1957) and Arnold Brecht, *Political Theory: The Foundations of Twentieth-Century Political Thought* (1959).

40. By way of illustration see the fruitful use that Kenneth N. Waltz makes of the traditional theoretical literature in his *Man, the State and War* (1959).

41. A recent instance is Robert A. Dahl, *A Preface to Democratic Theory* (1956).

42. Notably those issued by the Inter-University Case Program (1948–) and the Eagleton Foundation at Rutgers University (1958–).

The Revolution in Mathematics

MARSHALL H. STONE

We are in the midst of an intellectual crisis which has profound implications for education everywhere in the world. This crisis has arisen in conjunction with the development of science, and is the direct consequence of man's adoption of scientific ways of thinking and of acting. We shall not resolve its tensions until we accept science as an integral and all-pervasive part of our culture, not only at the material level but also in the tangent spheres of the intellectual life and of education. So suddenly has this crisis developed and so far do its effects extend that we are forced to recognize in it the symptoms characteristic of a major mutation in human culture. Even though imagination is inadequate to paint the transformed society which will at length emerge from changes but recently begun, we can nevertheless see somewhat more than dimly that only a few generations hence every aspect of man's life will have been radically altered. Already we can recognize how importantly the progressive formation of scientific habits of thought and action is influencing man's relation to his universe. Everything—his relation to his physical environment, his relation to time and space, his relations with himself and with the society in which he lives, even, we must add, his relation to the spiritual realm—is being viewed and treated under new aspects because man has found in science a new instrument for perceiving and understanding the conditions of his existence.

Already the changes thus wrought by science have outstripped the slow evolution of our systems of education. As a new world struggles to be born, we realize with more than a little concern that we are suddenly called upon to make a very determined effort toward bringing education up to the level of our times and orienting it as best we can toward a future vastly different from anything familiar to us from the past. The situation demands that we rethink our entire conception of education, in a spirit of seeking the truth about ourselves and the universe in which we live, and with the aim of embodying that truth in what we teach and in our manner of teaching it.

Twentieth-Century Developments in Mathematics

As part of this task of reviewing and revising the ideas underlying our educational practices, it is essential to examine the quiet revolution which has taken place in mathematics in our own times and to appraise its now enormous potentialities for bringing fundamental advances in every domain where reason and scientific thinking have roles to play. Here it is not so important for the mathematician to recall that for something like two and a half millenia mathematics has held an exalted place in education—and among the humanities, be it noted—as it is for him to delineate clearly and strikingly the remarkable developments which have taken place during the twentieth century in mathematics and in its connections with other disciplines—and to explain why they now put such a tremendous pressure upon us to modernize our teaching of mathematics and even to give it a new and more central place in the educational scheme. What has to be grasped by all who are interested in education is that our conception of the nature of mathematics has been revolutionized, our technical knowledge of the subject vastly enlarged, and our dependence upon it for scientific and technological progress enormously increased. Indeed, it is becoming clearer and clearer every day that mathematics has to be regarded as the corner-stone of all scientific thinking and hence of the intricately articulated technological society we are busily engaged in building. We can foresee a time in the not very distant future when a complete identification of science, logic and mathematics will be achieved: what has happened already in the case of mathematical physics, particularly the quantum field theory, foreshadows what will happen in the other branches of science as they probe more deeply into the complex relationships they seek to understand.

While several important changes have taken place since 1900 in our conception of mathematics or in our points of view concerning it, the one which truly involves a revolution in ideas is the discovery that mathematics is entirely independent of the physical world. To put this just a little more precisely, mathematics is now seen to have no necessary connections with the physical world beyond the vague and mystifying one implicit in the statement that thinking takes place in the brain. The discovery that this is so may be said without exaggeration to mark one of the most significant intellectual advances in the history of mankind, comparable so far as mathematics is concerned with only one other great discovery—the recognition by the Greeks that the empirical facts of

geometry fall into logical patterns which can be so amalgamated that the whole subject appears as a coherent logical structure based on a limited number of axioms. Not unrelated to this profound modern insight into the nature of mathematics are certain other achievements of the twentieth century. Thus it has been possible to determine quite precisely the connections between mathematics and logic, and to define the scope of mathematics in terms so broad and so simple that they appear to provide a nearly final answer to the question "What is mathematics?" It has been possible to see how the different branches of mathematics are united into one imposing whole and to reinforce the technical bonds which tie them together within the framework of a general definition. It has been possible to win our way to a new point of vantage from which we are able to appreciate the remarkable technical advances achieved in mathematics over the last sixty years as merely the prelude to greater accomplishments in the future. When we stop to compare the mathematics of today with mathematics as it was at the close of the nineteenth century we may well be amazed to note how rapidly our mathematical knowledge has grown in quantity and in complexity, but we should also not fail to observe how closely this development has been involved with an emphasis upon abstraction and an increasing concern with the perception and analysis of broad mathematical patterns. Indeed, upon closer examination we see that this new orientation, made possible only by the divorce of mathematics from its applications, has been the true source of its tremendous vitality and growth during the present century. We realize too that the trend toward abstraction must inevitably continue, reinforced by the successes which are already to be credited to it. In following this trend and directing their attention more and more to the discernment and study of abstract patterns, mathematicians have become increasingly aware of the fundamental antithesis between the structural aspect of mathematics and the strictly manipulative aspect which so often appears to have paramount importance for the applications and so often is the principal preoccupation of the mathematics teacher. A deeper understanding and appreciation of this antithesis has recently been achieved through some remarkable technical studies in modern logic which have disclosed inherent limitations upon what can be accomplished by the purely mechanical manipulation of a symbolic calculus. It is entirely obvious that these new insights and advances, which in sum constitute a genuine revolution in mathematics, pose difficult practical problems for the educator. Merely to incorporate into the mathematical curriculum the essential elements of our new mathematical knowledge is a formidable enough

task, but the necessity for presenting mathematics as the abstract subject it has become and reconciling its antithetical aspects greatly increases the difficulties involved in bringing mathematical instruction up to the level demanded by our times. Consequently, we need to examine in considerable detail the several factors which could be mentioned only briefly in these introductory remarks.

The Abstract Nature of Mathematics

The philosopher Bertrand Russell has described the abstract nature of mathematics in the epigram: "Mathematics is the subject in which we do not know what we are talking about or whether what we say is true." This is his way of asserting that mathematics is abstract—"we do not know what we are talking about"—and that the notion of mathematical truth is purely formal—we do not know whether what we say is true, in any factual sense. While Russell's description underscores the independence of mathematics from the phenomenal world it fails to define the content of mathematics. A modern mathematician would prefer the positive characterization of his subject as the study of general abstract systems, each one of which is an edifice built of specified abstract elements and structured by the presence of arbitrary but unambiguously specified relations among them. He would mean by the study of such mathematical systems not only the examination of the intrinsic properties of individual systems but also the comparison of the structures of different systems. He would maintain that neither these systems nor the means provided by logic for studying their structural properties have any direct, immediate or necessary connection with the physical world. At the same time he would recognize that such mathematical systems can often usefully serve as models for portions of reality, thus providing the basis for a theoretical mathematical analysis of relations observed in the phenomenal world. He would also acknowledge that this kind of accidental and to a certain extent arbitrary connection between some part of reality and a certain mathematical system has often led to the discovery of abstract features of the latter which could eventually be made the subject of abstract mathematical proof.

Naturally this view of mathematics, which differs so radically from the one held by the ancient Greeks or for that matter by all mathematicians prior to the nineteenth century, was not formulated at a single stroke but has evolved little by little over a fairly long period of time. The discovery from which our current view of mathematics as a totally abstract, strictly logical and entirely independent discipline has emerged was the

discovery of non-Euclidean plane geometry by the two Bolyais, Gauss and Lobachevski early in the nineteenth century. Gauss quickly sought to decide by experiment whether the geometry of space requires a Euclidean or a Lobachevskian model, but concluded that he could not decide the question without making measurements of a greater accuracy than he could attain. Thus two internally consistent but mutually incompatible mathematical systems were available for describing physical geometry. It was impossible to imagine that either of them could have at the expense of the other any necessary connection with the physical world. It was likewise impossible, at least for the time being, to choose between them on physical grounds. The subsequent development of many other kinds of abstract geometry hastened the emergence of the modern view which we have described here. Further impetus in the same direction came also from the side of algebra, where it gradually became evident that mathematics must deal not with a single number system but rather with an infinity of such systems, sharing many common features but at the same time having their individual peculiarities. A third contributing factor was the development of new techniques and concepts in logic during the last half of the nineteenth century and the early part of the twentieth, as a result of which it became possible to clarify the relation between mathematics and logic and to justify the modern definition of mathematics which has been given here.

Mathematics and Logic

Indeed, we have now reached the point where we can actually identify mathematics with logic. The central facts on which this identification is based were established in minute detail by Russell and Whitehead in their monumental treatise, *Principia Mathematica,* published in 1911. This remarkable work may be regarded as the culmination of a series of brilliant nineteenth-century contributions to the study of the connections between mathematics and logic. We refer particularly to the symbolic treatment of logic by Boole, Schroeder and Peano, the creation of set theory and transfinite number theory by Cantor, and the logical analysis of the real number system by Dedekind and Frege. Just as the Greeks had to develop logic in order to explore the implications which connect the empirical facts of geometry, so the mathematicians of the nineteenth century had to go still more deeply into logic in order to treat the new mathematics of their time with sufficient accuracy and precision. They were thus led to make many technical improvements and extensions in

the earlier logic and to understand the scope and nature of the function concept. As a matter of fact, from a mathematical point of view it is the growing preoccupation with this concept under one or another of the many names attached to it (e.g., function, operator, transformation, mapping) which is the key to certain of the trends characteristic of modern mathematics. With only a little oversimplification it may be said that what had to be added to the logic of propositions and the set theory of Aristotle and the scholastic logicians in order to make logic equal to the new mathematical demands upon it was the analysis of this concept, as worked out by the mathematicians and logicians mentioned above. Russell and Whitehead first showed how all of mathematics could be expressed in terms of a formal logic embracing the concepts of set and function. In this sense mathematics may be considered a part of logic. On the other hand, the full realization of Leibnitz's dream of reducing logic to a symbolic calculus was achieved in a succession of steps associated with the names of Boole, Peano, Russell, Whitehead and some later workers. In consequence we can now recognize that formal logic, being the study of operations upon appropriate symbols, must take its place as a special chapter in algebra and hence appears as a part of mathematics. The fusion thereby achieved between mathematics and logic reinforces the conclusion that mathematics is a completely abstract formal discipline, and raises interesting questions as to how much can be accomplished by the mechanical manipulation of symbolic systems and how much must depend upon direct or intuitive insights into their structural patterns. Indeed, it is clear that mathematics may be likened to a game—or rather an infinite variety of games—in which the pieces and moves are intrinsically meaningless and the absorbing interest lies in perceiving and utilizing the patterns of play allowed under the rules. When mathematics is viewed in this light, the questions just noted pose the problem of determining whether or not it is possible to reduce the play of one or another of these games to a prescribed automatic procedure, leaving no room for the exercise of judgment and inspiration. Thus the distinction between the manipulative and structural aspects of mathematics acquires a sharper meaning and a greater significance by virtue of the identification of mathematics and logic.

The Unity of Mathematics

The characterization of mathematics as the study of systems comprising certain abstract elements and certain abstract relations prescribed

among them shows very clearly the essential unity of mathematics. Nevertheless it cannot adequately suggest the intimate structural connections which have actually been found among the different branches of mathematics, as a result of modern researches. During the last fifty or sixty years much has been done to identify and compare the mathematical systems dealt with in algebra, number theory, geometry and analysis. The outcome has been surprising in two respects. On the one hand, the possibility of analyzing the mathematical systems known to us in detail has been exploited to the point where they are all seen to be derived from systems of three simple types: algebras, ordered systems, topological spaces. On the other hand, there have been discovered numerous processes, some of them very complex, for synthesizing new systems from given systems and using them to obtain information about the latter. Furthermore, many of the familiar systems—as for example, the number systems of analysis—can be characterized by the conjunction of simple algebraic and topological or ordinal properties. In a sense, therefore, the parts of mathematics about which we know a great deal are those lying fairly close to certain very familiar landmarks; and the parts which are not yet explored may very likely be of quite a different nature from those where we now feel more or less at home.

Even though, as a result, we have no warrant for supposing that the mathematical investigations of the future will necessarily use techniques or disclose phenomena generally like those known at the present time, we may still expect the new mathematics, however diversified, to be embraced in the unity laid down by the definition offered here. Indeed, this definition is so broad and admits so high a degree of abstraction that the limits it sets to mathematics can hardly be pushed any further back. We thus have good reason to believe that eventual modifications in our basic conception of the nature of mathematics must depend upon new developments in logic—developments involving new techniques and new points of view, which will grow out of our widening experience as mathematicians.

Abstraction and Application

It may seem to be a stark paradox that, just when mathematics has been brought close to the ultimate in abstractness, its applications have begun to multiply and proliferate in an extraordinary fashion. There is no doubt that one of the most exciting features of intellectual life in the twentieth century is the penetration of mathematics into an ever widening circle of scientific disciplines, not only the natural sciences but also those devoted

to the study of human behavior. As this penetration gradually leads to the beginnings of mastery over situations which had previously defied theoretical treatment, we have our first clear intimations of how far and how deep the writ of mathematics is destined to run. A whole new world of thought and understanding opens out before us to which mathematics alone is the key. Far from being paradoxical, however, this conjunction of two apparently opposite trends in the development of mathematics may rightly be viewed as the sign of an essential truth about mathematics itself. For it is only to the extent that mathematics is freed from the bonds which have attached it in the past to particular aspects of reality that it can become the extremely flexible and powerful instrument we need to break paths into areas now beyond our ken. The examples which buttress this argument are already numerous, and there is no doubt that they will be reinforced by many others during the second half of this century. We may mention the modern development of an elaborate mathematical theory of genetics, the recent creation of game theory with applications to economics and to a variety of other situations characterized by competition, and the still more recent formulation of a mathematical theory of communications with applications in engineering and linguistics. Compared with the mathematics required in the more difficult parts of theoretical physics, such as the quantum field theory, that used in some of the newer applications often seems simple and even unsophisticated. Very likely this appearance will change as more powerful mathematical techniques can be developed and the scope of the applications broadened out. Indeed it would be altogether realistic to predict that the general trend will be towards increasingly intricate mathematical theorizing in all parts of science, even in those branches where we cannot yet discern anything beyond the first tentative formulations of a few rudimentary mathematical principles.

If we are to paint an accurate picture of what is taking place in mathematics in our time, we cannot confine ourselves to these generalities, but must try also to give some account of the important technical advances which have been made during the twentieth century. No period in history has seen such intense and fruitful mathematical activity as has the first half of the present century. Moreover, this activity is now increasing very sharply indeed and will certainly continue to do so. In the last three or four years the number of active mathematicians, as measured in terms of publications, seems to have doubled. While this simple statistic might easily be twisted to give a distorted view of the situation, there is other

strong evidence that mathematics is experiencing a literally explosive growth. More significant than anything else is the increasing rate at which important new ideas and techniques are being introduced into mathematics. While it is a difficult task to do more than hint at the nature of the tremendous technical progress currently being made, it is one which we now propose to undertake, albeit with an inevitable sense of inadequacy.

Modern Algebra

Let us first look at algebra. By an algebra, or an algebraic system, we today mean a mathematical system comprising certain abstract elements and certain specified finitary operations applicable to them. In essence, an operation is identifiable with a functional relation; it is finitary if it is a relation among only a finite number of elements. Thus the various number systems studied in elementary mathematics—the integers, the rational numbers, the real numbers and the complex numbers—are algebras with two basic operations, addition and multiplication. Our modern conception of algebra grew out of the study of these particular systems by a process of abstraction and generalization, which has now reached its natural limits: if we proceed to consider relations which are not finitary operations, we find ourselves dealing with mathematical systems in the most general sense of the term and we lose contact with the guiding features suggested by the examples from which we started. During the twentieth century great technical strides have been made in the study of algebraic systems, so that even at the level of elementary instruction we need to revise and reorient the presentation of our algebraic knowledge. Needless to say, courses in algebra beyond the elementary ones are already vastly different from those offered fifty or even twenty-five years ago.

Equally striking has been the extensive formation of fruitful contacts between algebra and the other branches of mathematics. Today it is easy for us to understand why algebra has such an important role to play in other parts of mathematics, but history shows that mathematicians have been very slow in understanding this relationship and in learning how to exploit it successfully. In fact, every part of mathematics obviously involves the behavior of objects of interest to it under appropriate operations—that is to say, the mathematical systems which are to be studied are, in the nature of things, linked to certain algebraic systems. When these related systems are wisely selected and analyzed in the light of

general algebraic principles, important insights and information can usually be gained. In geometry, for example, the ancient Greeks were aware that they needed to study the properties of certain operations and, as a perusal of Euclid's *Elements* shows, they devoted a great deal of effort to solving certain algebraic problems in a geometrical manner. Lacking simple algebraic techniques and being unready for the abstraction required, they encountered difficulties and complications which disappeared once the role of algebra was explicitly stated by Descartes in the seventeenth century. The extent to which algebra is able to contribute to analysis was not realized until much more recently, in our own century. Again difficult problems, once they are viewed in terms of algebraic concepts, may become much more perspicuous and much easier to solve. Even formal logic, as we have already had occasion to observe, appears as a part of algebra by virtue of the fact that it deals with operations on symbols. While algebra has been very suggestive in this connection, its role in logic is not yet so important as in either geometry or analysis. Today it is unthinkable that a mathematician should pretend to a mastery of either geometry or analysis without a thorough grounding in the elements of algebra, especially group theory and linear algebra. Geometry, indeed, relies so heavily on these parts of algebra that the two subjects must be viewed as inextricably bound up together. Nowhere is this more evident than in the field of topology where such brilliant progress is now going on. It was no doubt inevitable that the close association of these two branches of mathematics should have reactions in algebra itself; and, as a matter of fact, procedures which first proved themselves valuable in combinatorial topology have now been taken over into algebra and have led to the creation of a new discipline known as homological algebra.

It was also inevitable that the increasing importance of algebra in the other parts of mathematics should be reflected in many fields where mathematics is applied. However, the contacts between algebra and applied mathematics are increasingly more direct than that, because there are so many situations in which the problems of applied mathematics have to be formulated from the start in algebraic terms. This is true, for instance, in the case of quantum field theory, as well as in those of circuit analysis, linear programming, and game theory, to cite a few of the more important examples. Consequently, it is not only the mathematician but also the applied mathematician who today needs a good grounding in algebra, especially group theory or linear algebra or both.

A special branch of mathematics which has always had the most intimate associations with algebra and might indeed even be considered a part of it, had not methods drawn from analysis been so extensively used there, is number theory. The study of the additive, multiplicative and other algebraic properties of the natural numbers—that is, of the finite cardinal numbers—has always had a tremendous fascination. Many of the problems of number theory can be very simply formulated in the mathematical terminology of common speech and are thus easily understood without much mathematical preparation. Among them are some of the most difficult unsolved problems in the whole of mathematics. Such problems attract the attention not only of serious mathematicians but also of amateurs and even of mere publicity seekers. Who would not like to solve Goldbach's problem: to show that every natural number is the sum of a limited number of primes (perhaps at most three)? The problems of number theory are not restricted to those concerning the natural numbers, as they have generalizations or analogues in other algebraic systems. Indeed, this fact has led historically to the development of a good many very useful algebraic concepts and techniques. In the same way the reduction of certain problems of number theory to problems of mathematical analysis has stimulated very deep work in the latter field. The success of analytical methods, while incomplete in the case of some of the most interesting and difficult problems, has nevertheless been sufficiently marked that in recent years a good deal of effort has been devoted to devising more elementary attacks. Some remarkable achievements, such as the elementary proof of the so-called prime number theorem, have been made along this line in the last few decades. Number theory is not totally without interest for applied mathematics, but it remains largely the preserve of the pure mathematician. Some of the analytical problems, and perhaps also some of the algebraic applications, associated with number theory do indeed have a certain intrinsic interest for fields of application, but this would hardly justify teaching number theory in an applied mathematics program. On the other hand, the inclusion of elementary courses on number theory in the mathematical curriculum, whether on technical or cultural grounds, certainly needs no defense.

Developments in Geometry

In our brief discussion of algebra, we have already noted how deeply geometry has been penetrated by algebraic concepts and techniques. There are some parts of geometry which have been completely taken

over by algebra. For example, the study of the sets defined by algebraic equations, originally taken up in relation to the real and complex number systems as a part of higher analytic geometry, is today purged of all analytical tendencies and is carried on by purely algebraic methods, applicable to much more general algebraic systems than the two classical number systems. Thus algebraic geometry is quite literally a part of algebra. Similarly, combinatorial topology, though basically concerned with continuity considerations, lent itself readily to algebraic treatment and as a result has been nearly completely swallowed up by algebra—though not without influencing algebra in the process, as we have already noted. In spite of these examples, the fact that geometry is concerned with continuity properties not very amenable to discussion in an algebraic spirit has protected that ancient branch of mathematics against complete domination by modern algebra. Consequently, some parts of geometry remain largely untouched by algebraic techniques. Since these parts are those where continuity considerations are prominent, and the outstanding problems often have an analytical aspect, there has been a tendency for them to gravitate towards analysis. Both differential geometry and general (or set-theoretical) topology illustrate this tendency. In differential geometry, of course, the ties with classical analysis are extremely close, most of the important problems leading directly to problems in the theory of differential equations. Geometry therefore has been strongly pulled in two apparently opposite directions and has seemed at times to be under the threat of being torn asunder. More recently, however, a closer study of the fundamental concepts of differential geometry has begun to produce a new synthesis of the algebraic and analytic points of view, made somewhat easier by the fact that the roles of algebra and topology in analysis have come to be better understood and appreciated. For some time, mathematicians have been groping for a satisfactory way of effecting such a synthesis, and there are now rather plentiful indications that their search has been at least moderately successful. In any case, there is no doubt that difficult problems involving algebraic, analytic and topological features can now be clearly formulated and neatly solved. The prospects for a brilliant development of these complex aspects of geometry now seem to be assured. This circumstance in itself creates a difficult problem in mathematical instruction because we must evidently provide suitable introductory material to enable future mathematicians to make or at least to understand progress in these promising directions—and we do not yet quite know how to do this.

Mathematical Analysis

Since mathematical analysis, like geometry, has to deal with situations in which continuity considerations and limit processes play a dominant role, there has been a double impetus toward the abstract treatment of such processes. Thus there has emerged the concept of a topological space as a mathematical system consisting of abstract elements called points and a single limit relation among them, specified in any one of several essentially equivalent ways. Historically, the introduction of this concept was associated with the calculus of variations and thus had an analytical rather than a geometrical background. The subsequent development of general topology—that is, of the theory of topological spaces—and of the related parts of analysis known as functional analysis or abstract analysis has been a particularly characteristic and important achievement of twentieth-century mathematics. While it has thrown much light on the foundations of both geometry and analysis, we must today regard general topology as a part of geometry rather than of analysis because its major problems seem to have greater significance for geometry. As a matter of fact, the mathematical systems which the analyst finds genuinely interesting have linked algebraic and topological properties and cannot be adequately discussed in terms of general topology alone. Indeed, it would be possible to define modern abstract analysis as the study of topological algebras—that is to say, of mathematical systems which are simultaneously topological spaces and algebras with operations continuous relative to the underlying limit processes. A great deal of classical analysis has recently been brought into close relation with the theory of topological algebras, and many classical problems have thereby been shown in a new light and brought nearer to solution. Thus the study of differential equations has been formulated in terms of topological linear spaces (or vector algebras), and harmonic analysis brought under the theory of commutative topological groups. Many of the noteworthy advances being made currently in the theory of partial differential equations are actually based on the application of methods drawn from the theory of topological linear spaces. In the other direction, it has been possible to show that certain topological algebras are describable in terms of classical analysis. The most spectacular result of this kind was the recent solution of Hilbert's fifth problem: to express the operations of a topological group in terms of analytic functions. Although analysis has thus been very strongly influenced by the abstract

tendencies of modern mathematics, it retains a very lively interest in a variety of special problems, special functions and special functional equations, particularly some of those related to applications. As a result this branch of mathematics must be taught with particular care so as to give due emphasis to both its traditional and its modern aspects.

Computation

In connection with the applications of analysis great importance has always attached to the execution of extensive computations without which the passage from theory to practice would be impossible. As the other branches of mathematics begin to play a bigger part in the applications, the scope of computation needs to be correspondingly enlarged. Under this pressure for more powerful and efficient computing instruments extraordinary progress has been made since the mid-thirties in developing machines of astounding speed, capacity and versatility. The result has been a revolution in the art of computation and a complete alteration of the relations between mathematics and that art. Computations which a few years ago would have been considered quite beyond the limits set by practical considerations can now be carried out in a few days or weeks, if not hours. There is no finite sequence of mathematical or symbolic manipulations which cannot be effected, in theory at least, by a general-purpose computer. The practical limitations upon the length and complexity of the sequences which can be handled by today's machines will almost certainly be pushed back as the electronic and other physical techniques applicable in computer design are brought to perfection. With the introduction of these powerful aids, it is no longer the mathematician's task to strive for a reduction of his solutions to a form readily computable by hand or with the aid of the simplest calculating machines: it is his role to translate them into programs for execution by high-speed electronic computers. In consequence, there is being created a new specialized branch of mathematics devoted to the theory and practice of setting up computer programs. It requires a knowledge of the mathematical techniques involved in computation and of their logical combination, and has obvious contacts with formal logic. Indeed, one can see—after the event—that the entire development of modern machine methods in mathematical computation has been made possible by two basic advances—Russell and Whitehead's discovery of the reduction of mathematics to formal logic and the invention of the vacuum tube. Accordingly, the emerging ties between the computing art and logic are

in no sense accidental and will undoubtedly become closer and more intricate with the passage of time.

The versatility of today's computing machines not only makes possible mathematical and logical analyses which one would hardly have dared to imagine a few years ago, but has also focused our attention sharply upon that antithesis between manipulation and pattern-perception to which we have already referred. As we realize how much can be done by machines, we have to ask whether it would be possible to design one which could do everything done by the brain. This amounts, on the theoretical side, to asking whether the brain itself is a machine. Another way of putting the question is to ask whether all of mathematics and logic can be reduced to the execution of a routine program of symbol-manipulation, eliminating the need for the direct discovery and utilization of patterns. Although, as will presently be seen, the modern study of logic suggests that our instinctive negative reply to this question may be correct, it is sobering to realize how much can be accomplished by purely mechanical procedures. Already there are machines which can state and prove simple mathematical theorems or outplay a human opponent at a simple game like checkers, even learning from experience to avoid the repetition of past mistakes. It is no wonder then that one of the surprising uses of the new computers is to assist the sociologist in analyzing human behavior, by simulating the reactions of individuals to one another in accordance with the hypothetical principles which are to be tested. Here, as in many other applications of the new machines, the capacity for carrying out at high speed an incredibly large number of operations is essential, because only thus can the consequences of varying a relatively large number of variables and parameters be explored. This is the reason why the modern computers are especially useful whenever the number of variables and parameters entering into a problem is too large for the application of simple formulae and not large enough for a resort to asymptotic estimates. For instance, the practical solution of many problems in mathematical economics or linear programming would be impossible without the help of modern computing machines, not because the mathematics employed is difficult or complicated but because the variables involved are usually so numerous.

Modern Developments in Logic

In view of the foregoing remarks upon the relations of computing theory to logic, we shall not be making an abrupt change of subject if we

turn at this point to examine some modern developments in logic. Having already dwelt upon the fundamental contribution of Russell and Whitehead, we must now consider a line of enquiry laid down by Hilbert in 1900, when he included in his eventually celebrated list of important problems that of demonstrating the consistency and completeness of at least one substantial portion of mathematics. Thirty years later Gödel shocked the mathematical world by showing in a brilliant paper that if a system comprising the so-called first order functional calculus of logic and the axioms of arithmetic is consistent, then it must be incomplete in the sense that there is a proposition of arithmetic which can be formulated within it but which can neither be proved nor disproved in it. A related and equally intriguing phenomenon was discovered by Church and Turing who showed that it is impossible to prescribe a systematic procedure capable of deciding whether each particular proposition in this system is provable or not. In order to express this result in a particularly precise and vivid way, Turing chose to describe a certain imaginary machine and to demonstrate its limitations. He was thus among the first to point out the connection between a fundamental problem of logic and the theory of computing machines. It is, in fact, this result of Church and Turing which suggests that logic and mathematics cannot be reduced entirely to a routine program of mechanical manipulations with symbols, that there may be some things possible for the human mind which any particular machine cannot do. Thus the antithesis we have emphasized between manipulating symbols and perceiving patterns may perhaps be related to the logical phenomenon discovered by Church and Turing, and may be genuinely rooted in the distinction between mind and the machine.

Probability and Statistics

Just as logic arose from the study of deductive reasoning, so the theory of probability and statistics originated in the attempt to formalize the processes of inductive inference. The theory dates from the sixteenth century, when the first mathematical analyses of games of chance were made, and it was quite highly developed by the end of the nineteenth. Successful applications had by then been made to the theory of errors of measurement, the theory of gases, and a good many biological and actuarial situations. The twentieth century has witnessed extensive technical developments in probability theory and statistics and the rapid multiplication of their applications. As purely mathematical theories they

can now be identified as a part of measure theory or, equivalently, of integration theory. On the other hand, as branches of applied mathematics they have tremendously increased importance and scope. Full understanding of their epistemological significance was not gained until the present century, but we can now recognize their unique and fundamental role as tools for knowing the phenomenal world. Increasingly, the models employed in our attempts to analyze man and nature are statistical models. Even the deterministic models, such as those which have survived from classical physics, are admittedly asymptotic simplifications of more realistic statistical models. In consequence the student of any of the sciences, physical, biological or behavioral, now needs a good grasp of the elements of probability theory and statistics. Thus there is posed the problem of how and in what form modern courses in probability theory and statistics should be introduced into the mathematical curriculum. There is increasing support for putting the first introductory treatment at a very early stage so that our students may become acquainted as soon as possible with the elementary principles of statistical reasoning, now so prominent in a wide range of human thought.

No survey of the recent history of mathematics so brief and so topical as the one we have attempted here can possibly give an adequate picture of the scientific development of modern mathematics. It may, however, serve to persuade the general reader of the truly revolutionary and intellectually significant character of that development and to give him a useful background for the discussion of some of its educational implications. By reviewing, even if much too summarily, the important trends in modern mathematics and some of the advances which are in progress in its various branches—algebra, number theory, geometry, analysis, computation, logic, and probability theory and statistics—we have perhaps been able to suggest the spirit and the substance of the extensive changes which have become necessary by now in our teaching of mathematics.

The Central Problem of Mathematics in Education

As we have put it already, mathematics does not seek an essentially new place in the curriculum. It has always held a place of honor and importance there, both as a humanistic study and as a useful scientific discipline. What it now seeks is to adjust its place in education in accordance with its growing importance in the intellectual life of our times. In such an adjustment the central problem is to define a new core of mathematical knowledge embodying the leading ideas and the principal techniques of

modern mathematics and to organize the teaching of this core as a well-articulated program, taking advantage of whatever light modern psychology can throw on intellectual development, concept-formation and the learning process. Subordinate to this problem, but none the less of first importance, is the problem of grouping as satellites around this mathematical core a number of more specialized bodies of fundamental knowledge, each basic to some special purpose such as preparation for teaching, for the pursuit of higher mathematics, or for work in some field of applied mathematics. Another difficult and challenging problem is that of coordinating the teaching of pure mathematics, as represented by the core and its various associated bodies of more specialized mathematics, with the teaching of the widely different fields in which mathematics is being applied. This problem, already a critical one so far as physics is concerned, seems certain to command a great deal of attention during the coming decade. It is made all the more pressing because of the abstract tendencies apparent in present-day mathematics and its involvement with the ever more sharply defined antithesis between the manipulative and the structural aspects of mathematics.

To open the discussion of the central problem, let me offer three poems —all scientific poems—which together occupy but a single line:

$$E = mc^2, \qquad E = h\nu, \qquad \alpha(Aa) = (\alpha A)a.$$

Like Chinese poems they must be taken in by the eye as well as by the ear. The first will be recognized at once as the famous equation between energy and mass, epitomizing a whole revolution in physics; it was discovered by Einstein early in this century as an essential principle of the theory of relativity. The second likewise states a revolutionary principle of modern physics, proposed by Planck shortly after the turn of the century and made the cornerstone of the quantum theory; it expresses a relation between energy and the frequency of vibration of a wave of light. An understanding of these two poems is impossible without a reasonably good grasp of the core of modern physics. Indeed, if one were to set out with the single aim of teaching their meaning and their implications, he would end by giving his students a very satisfactory introductory course in physics. In the same way, the last of these three poems is an epitome of a large part of what should be taught as an introduction to modern mathematics. Even for a mathematician this statement might remain obscure, unless he were given a hint to interpret the equation as symbolizing Stokes's theorem; and he would then understand at once just

how much is needed from algebra, geometry, topology and analysis before the significance of this equation can be fully grasped. As it stands, the equation merely expresses a kind of abstract associative law, analogous to the one which is verified in case the three symbols α, A, and a designate cardinal numbers. It is when we give altogether different interpretations to these same symbols that our equation becomes an expression of Stokes's theorem. Specifically, we should take a as designating a topological chain with numerical coefficients and boundary chain Aa (usually denoted as ∂a), α as designating an exterior differential form with differential αA (usually denoted as $d\alpha$), and βb as designating the integral of the exterior differential form β over the chain b (with respect to the appropriate geometric measure on the latter). The symbol A must then be thought of as designating a double operator which sends a into Aa and α into αA.

Whether we actually start in this or in some other way to define the core of the mathematical curriculum is of no great importance. Our goal is the same in any case: to list those topics from the various branches of mathematics which we deem significant enough and central enough to be taught to everyone seriously interested in gaining a mastery of the essential elements and techniques of modern mathematics. If we were to examine the current discussions of what these topics might be, we would find a good deal of evidence for believing that agreement might be reached on a list made somewhat as follows: from algebra—the topics of linear algebra, group theory (with an elaboration of some of its ramifications in the theory of rings and fields) and elementary number theory; from geometry—the treatment of Euclidean, analytic (properly, algebraic), projective and differential geometry together with some kind of introduction to elementary topology; from analysis—the calculus, an introduction to the theory of differential equations, the elements of the theory of analytic functions of one complex variable, and an introduction to functional analysis; from the field of computation—elements of the theory and practice of numerical analysis, the central role of approximate numerical solution of problems in linear algebra; from logic—the elements of set theory including cardinal and ordinal numbers, the algebra of logic (Boolean algebra) and an introduction to symbolic (mathematical) logic; from probability theory and statistics—the fundamental principles of inductive or statistical reasoning, probability distributions and their relation to measure theory, the principal statistics and their applications, and an introduction to the study of stochastic processes. The arrangement of such topics in a suitably articulated pattern of courses would require a

detailed examination of their interrelations and could not be satisfactorily accomplished without a good deal of study and debate. It would be against the spirit of modern mathematics to obscure or conceal the fundamental unity of the subject by too much compartmentalization of the listed topics in some kind of logical scheme. The aim should be to bring out that unity by showing how the main branches of mathematics touch and penetrate one another even in their most elementary aspects. Naturally, many topics mentioned here have different degrees of importance for different classes of students—those interested in general knowledge, the future teachers, the future professional mathematicians and the future specialized scientists. Accordingly, some of the courses offered as belonging to the central core of the curriculum should not be uniformly stressed in all circumstances or for all students. The special interests of special groups of students must be catered to in special courses giving adequate introductions to the various fields of specialized mathematical knowledge which cluster around the core we have tried to describe. This is hardly the place to analyze more closely either the core or these associated special fields, and we shall not attempt to do so.

Problems for the College Educator

Instead we may now turn to a brief consideration of a few specific problems which the college educator seems certain to face in the rather immediate future. We shall touch only upon such problems as arise in one way or another from the progress of mathematics, strictly excluding all those which have other origins. From what has preceded it is evident that among the most important of these problems are the following: first, to decide what part of the central core should be taught by the college and how that part shall be elaborated into a suitable course program; next, to decide what additional specialized courses should be offered by a particular college mathematics department; and finally to bring about adequate coordination between the instruction offered by the college mathematics department and the courses involving mathematics but falling under the jurisdiction of other departments.

In spite of the fact that mathematicians concerned with the general development and major trends in mathematics do have a reasonably good idea of how the central core is constituted at the present moment, that idea is not at all adequately expressed in the programs in mathematics currently offered by schools, colleges and graduate departments. In recent years, however, the need for a general reform leading to the mod-

ernization of the mathematics program at all three levels has been widely felt, and active steps have been taken to meet this need. While our colleges have not neglected this problem, it seems clear that more substantial progress has been made towards solving it at the high school and graduate school levels. So far as the teaching of mathematics is concerned, our colleges therefore find themselves under the influence of the changes contemplated or already initiated both above and below. The practical effect of these changes is to shift a good deal of mathematical material downward to a somewhat earlier point in the student's experience. In fact, the graduate school presses on the college to offer somewhat more advanced courses, including some now generally offered at the graduate level, so that their students may begin graduate study better prepared and arrive more quickly at the more advanced points of our present mathematical knowledge. On the other hand, the high school is starting to eye some of the subjects traditionally taught in college as possible material for courses in the junior and senior secondary school years. For example, some high schools have been teaching the elements of the calculus for many years, and there are many indications that before ten years have passed a majority of our larger high schools will do likewise. Thus the college educator is faced with the problem of up-grading the program of his mathematics department and staffing the department with more highly qualified teachers. Assuming for the sake of our discussion that it will not be long before the standard first-year college course in mathematics will be a thorough and rigorous treatment of the integral and differential calculus taught to students who already have had a more or less intuitive introduction in high school, we can reasonably expect that a great many of our larger colleges will be offering to juniors and seniors good introductory courses in the theory of analytic functions of a complex variable, modern differential geometry, and functional analysis, of the kind now given in graduate schools. If the upper and lower limits of the college mathematics curriculum should be fixed in some such manner, then the main problem could be narrowed down to that of elaborating the essential courses which are to be taught in this range. Here a great deal needs to be done—so much, indeed, that a concerted attack may very well be called for along the lines suggested by what is being done in the field of high school mathematics. It may be mentioned in particular that by and large the problem of teaching enough modern algebra at the college level remains open—and demands careful study by college mathematics teachers. Even the complete success of the current efforts to

modernize the teaching of algebra in the high schools will not mean that very much modern algebra will be taught there, for the simple reason that the main part of high school algebra as it is conceived of at the present moment is hardly algebra in the modern sense, being concerned mainly with the elementary analysis of real or complex polynomial functions. The determination of what should be taught at the college level in geometry is another problem which is far from being satisfactorily solved, as we have already mentioned in our earlier remarks. Indeed, we must expect that our ideas about this problem will shift a good deal over the next decade, in response to the progress being made by research in the field.

Clearly, the college can hardly undertake a richer and more advanced program in mathematics unless it is willing to eliminate a considerable amount of deadwood from the traditional college curriculum. There are a good many obvious candidates for elimination, subjects which should either be taught in high school or not be taught at all. Let us cite college algebra, solid geometry, most of numerical trigonometry, descriptive geometry and some topics in the calculus. Since a great many—perhaps a majority—of our colleges are still teaching these subjects, the work of reform has to begin by banishing them from the college curriculum. Only when this has been accomplished can the college mathematics program be given its proper scope and brought to the proper level of quality.

So far as the courses offered as a specialized adjunct to the core program are concerned, it must be expected that there will be great variation from one college to another. The professional interests of the student body and the professional qualifications of the teaching staff will both have an influence on this offering. It is easy to mention a number of special subjects which could quite appropriately be taught for college undergraduates, such as numerical analysis and computational techniques, mechanics, introductory mathematical physics, logic, game theory, linear programming, and so on. Few colleges can afford to offer a complete range of the possible choices, and there is certainly no reason why any college should even attempt to do so. On the other hand, the interests of the students certainly demand that some courses of this nature be made available, either by the mathematics department or by some other departments in which applications of mathematics are discussed. Thus the college educator who is mindful of the modern developments in mathematics and of the proliferation of its applications will actively encourage his faculty to study what can be done in terms of the combined resources

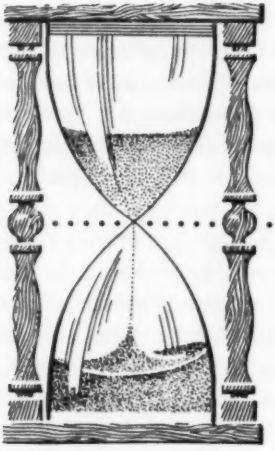
of the appropriate departments in order to provide adequate instruction in some of the more specialized parts of mathematics.

In any case, this problem is closely allied to the problem of coordination which, as we have already pointed out, can hardly be avoided by the college educator of today. Something more than encouragement may be required to bring about an adequate contact between the departments concerned. Certainly our experience suggests that close and continued cooperation between two departments is difficult to maintain without some outside compulsion or supervision. At the present time, there are some fields where the circumstances seem unusually favorable to initiating a serious cooperative effort to correlate the curricula and teaching programs of certain departments. This is undoubtedly the case so far as mathematics and physics are concerned. If these opportunities are to be seized upon and exploited, the college president must therefore not hesitate to exert strong leadership, it seems to me. It is quite evident that any success he may be able to achieve in making coordination a reality will mark a real educational advance for his institution. The nature of the problem of coordination is not hard to define and the means to its solution lie close at hand. The greatest obstacle to achieving a solution is the human difficulty of maintaining cooperation in a situation which manifestly demands it despite marked differences in points of view. Some attempt to reconcile these differences should therefore be made as an initial step in establishing cooperation between the mathematics department and any other department which may happen to be concerned. Instead of going directly to a consideration of the details of how specific courses should be interlocked, it would be better to undertake first a joint examination of the general state of the two subjects and of the points of view held concerning them by the members of the two departments. In most cases the basic difference of opinion is very likely to rest upon value-judgments about the antithesis between the concrete and the abstract or the related mathematical antithesis between manipulation and pattern-discernment. Since abstraction and the discernment of patterns are demonstrably playing more important roles in the scientific study of nature—as is very strikingly plain in the case of quantum field theory—the mathematician's desire to emphasize the abstract and the structural aspects of mathematics, both because of their intrinsic interest and because of the firmer grasp they give of concrete situations and manipulative techniques, may today begin to be more sympathetically received than was formerly the case. On the other hand, it is important that the

mathematician should not neglect the manipulative aspects of his subject nor insist that his own concern for generality and precision should at all times and in all circumstances be shared by those who engage in making applications of mathematics. Granted that some mutual understanding can be reached on these points, the actual coordination of the teaching of mathematics with that of other subjects can be undertaken in a favorable atmosphere. The mathematics department should do its best to offer the right topics at the right time, with some orientation toward the applications which may be envisaged; and on the other hand, the other department concerned should endeavor to use, and use correctly, the preparatory materials furnished by the mathematics department and should assume some obligation to give its students further practical training in the mathematical principles and techniques involved. Obviously, such coordination as we have thus described demands much hard and patient work for its realization. It cannot be achieved overnight or without the maximum of good will and of dedication to what should be one of the most important educational purposes of our time—the purpose of bringing out the essential unity of human thought.

Summary

To summarize, then, we have seen how mathematics has undergone revolutionary changes and an explosive development during the twentieth century; we have seen how it has simultaneously multiplied its contacts with other fields of investigation, assuming in many of them a key role and in others at least an indispensable auxiliary one; and we have identified many serious educational problems which stem from the resulting urgent need for modernizing our teaching of mathematics. These problems are particularly acute at the college level and constitute a great contemporary challenge to the wisdom and the energy of our college faculties and college presidents.



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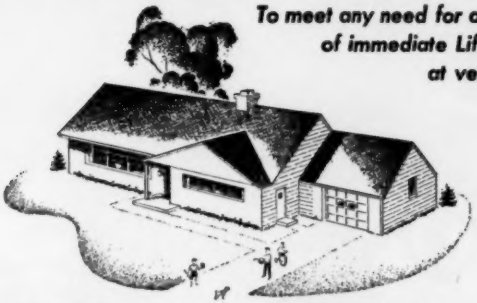

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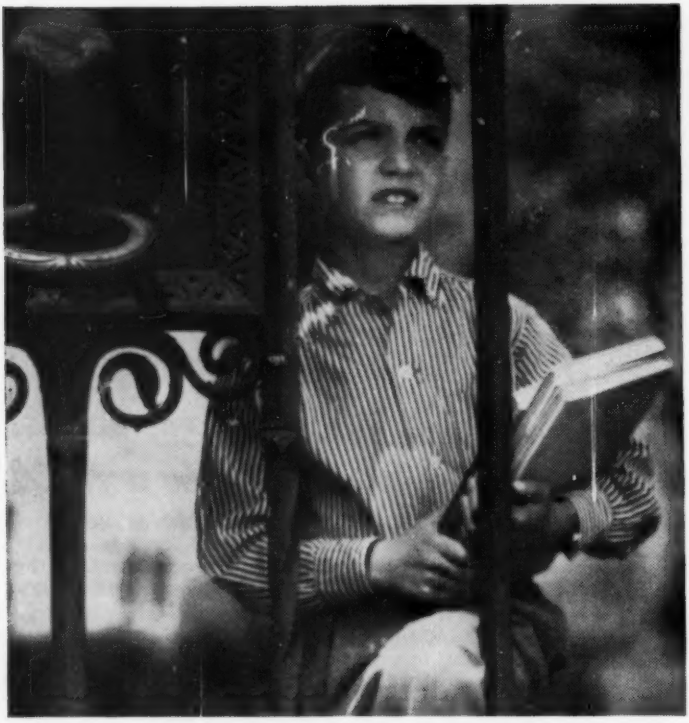

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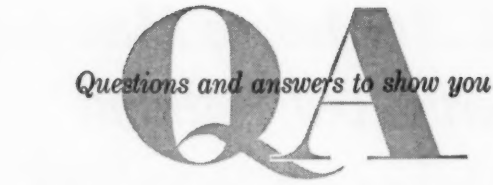

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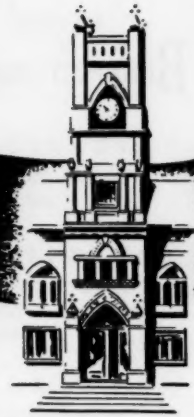

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